THURSDAY, OCTOBER 5, 1871

OBSERVATIONS UPON MAGNETIC STORMS IN HIGHER LATITUDES

THE extension of the telegraph into the more northern latitude of the Shetland Islands, between 59° 51′ and 60° 51′ 30″ N., has afforded a much better opportunity of observing the frequency and variation of the magnetic and auroral storms that have of late excited some attention and discussion in these pages.

Some of the earliest recorded observations upon the strength and direction of these atmospheric storms, date from the time when the extension of the telegraphic wires over England rendered the phenomenon visible by the disturbance of the magnetic needle placed in circuit with the wires, and to a certain extent rendered possible the mapping down of the position and direction of the magnetic storm over certain tracts of Great Britain.

On the 24th September, 1847, remarkable magnetic disturbances were observed in London, and the direction and deflection of the magnetic needle noted. The effects of this magnetic storm were carefully observed at Dawlish, Norwich, Derby, Birmingham, Rugby, Cambridge, Tonbridge, Wakefield, Edinburgh, and York. The magnetic disturbance appears to have commenced about $1^h\ 5^m$ P.M. on the 24th, and continued with variable intensity until $7^h\ 30^m$ A.M. on the 25th.

It may be interesting to give some of the galvanometer readings recorded as indicating the rapid oscillation and deflection of the galvanometer needle. In the period of time between $4^h\ 17^m$ P.M., and $5^h\ 48^m$ P.M. on the 24th, or in about one hour and a half, the direction of the current had changed no less than ten times, showing a maximum swing of the needle over an arc of 50°.

H. M.	deg.	H. M.	deg.
4.17	15 left	5.5	15 left
4.20	20 right	5.11	12 „
4.25	1 „	5.16	10 right
4.25.30^s	18 „	5.22	18 left
4.35	6 „	5.25	14 right
4.38	12 „	5.28	13 left
4.45	20 „	5.32	20 „
4.50	10 left	5.34	26 „
4.51	17 „	5.42	29 „
4.55	0 „	5.48	30 „
4.56	8 right		

During this magnetic storm, the variation of the dipping needle which was observed in London every 30^m, ranged between 69° 30′ and 67° 50′.

In some cases these magnetic storms were so severe as to impede the working of the railway signals. On the 18th of October, 1841, a very intense magnetic disturbance was recorded, and amongst other curious facts mentioned is that of the detention of the 10.5 P.M. express train at Exeter sixteen minutes, as from the magnetic disturbance affecting the needles so powerfully, it was impossible to ascertain if the line was clear at Starcross. The superintendent at Exeter reported the next morning that some one was playing tricks with the instruments, and would not let them work.

It will be fresh in the memory of many of our readers that during the month of October last year, very remarkable and brilliant "auroræ" were observed in London, chiefly of a deep blood-red colour, spreading from the zenith over a great portion of the heavens.

It is, however, in the more northern latitude of the Orkney and Shetland Islands that the grandeur of these wonderful electrical phenomena can be observed, and that reliable data can be obtained from which hereafter some practical result may be deduced.

As observed in Orkney and Shetland, the aurora, as a general rule, appears to concentrate and emerge from behind a dense mass of dark cloud lying low down in the horizon towards the north. The edge of this cloud-bank is serrated and jagged, as if the mass were electrically in a high state of tension. From behind this cloud-bank "dark" streamers will appear to start up high into the zenith, appearing as if attenuated portions of the edge of the cloud-bank had been dragged by some invisible power, these dark auroral rays being at the same time transparent as regards the power of transmitting the light of the stars, which shone through with undiminished splendour. At the same moment that these dark rays are emicant, brilliant green, violet, crimson, and white rays appear to stream upwards towards the zenith, but always with a less persistence of duration. These coloured scintillations change with greater rapidity than the black rays.

During the month of December of last year, some very vivid prismatic tints were observed from the Island of Eday. From careful observation it was then remarked that the red coloured rays appeared generally to be of a partially opaque nature, and it could be readily seen that the light of a star, when viewed through the red scintillation, was dimmed as compared with the brilliancy of the same star when observed through the scintillations of another colour.

In some of these displays, the most vivid and varied colouring was exhibited. These were noted down as visible to the eye at the same time, and as the colours were observed in contrast, the distinctiveness and brilliancy of the tint became the more decided. Black, pale yellow, strong yellow, white, violet, pale blue bright green, crimson shade fading into a reddish pink, pale orange, and a delicate sea-green tint. So far nothing approaching to the indigo hue has been noticed. With this exception, the entire prismatic colours and blending tints may be said to have been perfectly developed in the rapid electrical scintillations of the aurora. The colours fade away and change with astonishing rapidity, and this variation in tint will take place without apparently any great electrical disturbance in the special ray observed, beyond a slight flickering motion. In these regions, where the atmosphere is so perfectly still and at times calm, repeated observation has determined the existence of very appreciable sound to the ear, as an accompanying phenomenon [to the rapid rush of the auroral streams towards the zenith. The intensity of the sound emitted varies considerably. At times, it greatly resembles that of the rushing noise caused by the firing of a rocket into the air when reaching the ear from a distance. At other times it has a strong resemblance to the sound produced by the crackling of burning embers, but wanting in any very distinctive sharpness.

In all these cases of auroral displays the inductive effects upon the telegraph wires are very strongly marked; currents of varying intensity and direction flowing unceasingly through these metallic circuits.

The result of observations made in Shetland during the months of September, October, November, and December last year, tend to show that these auroral disturbances attained their maximum effect upon the wires between $8^h\ 30^m$ and $9^h\ 30^m$ A.M., and between $8^h\ 30^m$ and $10^h\ 30^m$ P.M.; and such is the unstableness of these induced auroral currents, that frequently in five minutes the electromotive force will vary from very much less than that of a Daniell cell to a current of such intensity that a brilliant stream of light will flash across the points of the lightning conductors with sharp detonating reports, the electromotive force of which would be scarcely equalled by 500 Daniell cells.

In January last very curious electrical phenomena were observed at Lerwick through the day-time, in connection with the N.E. gales so prevalent at that period of the year. In Shetland these gales are almost without exception accompanied with very severe hail-storms. The day begins bright and fine, a clear sky, the barometer rapidly rising; low on the horizon may be observed dense and angry-looking clouds. One by one these clouds travel fast towards the zenith, when all at once a fearful gust of wind, accompanied with the most violent hail-storm, will apparently break out of the cloud, and continue for about fifteen minutes. The wind then subsides, and the day appears as fine as before. In half an hour's time a second cloud will have appeared, and there will be a repetition of the temporary tornado and hail-storm. The remarkable circumstance attending these successive storm clouds is that they appear to be a purely electrical phenomenon. The moment that the icy discharge takes place from the cloud with its accompanying "crack" of wind, an induced electrical current appears upon the wire, so strong that it attracts firmly down the armatures of the telegraph Morse apparatus. The moment, however, that the hail ceases, the current passes off, but with this result, that each successive cloud storm appears to induce a current flowing inan opposite direction from the last, that is to say, the currents appear to be (using conventional language) positive and negative in their effects.

That these storms are "electrically excited" there is no disputing, and that they occur during the prevalence of the chief auroral displays is also a matter of observation, but so far their connection with aurora has not been sufficiently determined to permit any opinion to be expressed.

The recent successful completion of the telegraph circuit to Shetland, and the extensions immediately to be carried out one hundred miles farther north, will afford much greater facilities for auroral observation than has hitherto existed. It is also proposed to institute a careful spectroscopical examination of the coloured scintillations; and now that the Meteorological Society are about to establish an observation station in Shetland, there is every prospect of some valuable data being collected on this interesting subject, which may hereafter guide our meteorological students in arriving at some satisfactory conclusion regarding the laws of electrical storms and auroral induction. At present we are only able to record a few carefully observed facts.

THE LIGHT OF JUPITER'S SATELLITES

Ueber die Helligkeitsverhältnisse der Jupiterstrabanten, von Dr. R. Engelmann, Observator der Sternwarte zu Leipzig. (Leipzig; London: Williams and Norgate. 1871.)

OF all the satellite systems which so essentially enrich the retinue of the sun, none, when we have left our own moon behind us, promises such a reward for investigation as that of the planet Jupiter. The remoter ones may be, and probably are, intrinsically of a more remarkable character, but they are, and ever will remain to a great extent, beyond our reach; while the attendants of the largest among the planets are numerous enough to interest by individual peculiarities, which their comparative proximity enables us to study with advantage. Yet it is readily observable that though ordinary telescopes of good quality would have done much towards elucidating their phenomena, very little progress has been made in the inquiry, especially in this country; and the work now before us is the first attempt to collect and to make serviceable the scattered observations which exist, of which we are sorry to remark how few are due to the astronomers of England.

The especial object of the eminent observer at Leipzig has been not the theory of the motions of these satellites, but simply their physical aspect in regard to the variable light which they have long been known to reflect, and to this investigation the author, notwithstanding constant engagement in important zone observations, has contributed far more than all who have preceded him. The instrument which he employed was the astrophotometer of Zöllner. In this ingenious contrivance, the light of the object to be examined is referred to that of one or more known comparison stars, by means of an artificial star produced by a petroleum flame, adjustable for brightness and colour by a Nicol prism, and a "colorimeter," or revolving wheel of tinted rock-crystal. But in order to eliminate the effect of unequal areas, so as to ascertain, not merely the absolute amount of light reflected, but the "albedo," or reflecting power of each surface, it is, of course, necessary to obtain reliable measures of these minute specks of light; and in order to decide the interesting question whether or not their rotation and revolution are, as with our own satellite, synchronous, their anomalies, or orbital positions relative to their primary, have to be taken into account. All this has been done with most praiseworthy care; the whole is discussed and reduced with scrupulous and exemplary attention to every possible source of accidental error; and the result is given to the eye in several elaborate diagrams. We shall merely specify some of the conclusions, which will be found of considerable interest to astronomers. The absolute brightness was found by the author, as it has been by all previous observers, very variable; and from the irregularity and occasional rapidity of its changes, it becomes impossible to decide, in the case of the three interior satellites, whether the periods of rotation and revolution are identical. This, however, appears to be decidedly the fact with the outermost. Herschel I. had extended the inference to all of them; but such a result could not now be accepted; and it seems probable that the spots which must occasion these variations, and which have been repeatedly noticed when the

satellite has been on the disc of Jupiter, and by Dawes and Secchi even in other positions, may be of changeable character. At a mean II. is relatively the most, IV. the least luminous. As to their micrometrical measurement, every one who is acquainted with the telescopic aspect of these minute discs will readily comprehend its difficulty. It has, however, been attempted in various ways, but not by the double-image micrometer, which does not seem to have been used; the results, as may be expected, present considerable discrepancies, but the final values obtained by a combination of different methods in the hands of various observers are as follow:—I., 1″·081; II., 0″·910; III., 1″·537; IV., 1″·282; or, in English miles, 2,498, 2,102, 3,551, 2,962, the solar parallax being taken as 8″·90. These values, all things considered, differ so little from those given by Lockyer (Guillemin's "Heavens")—namely, 2,440, 2,192, 3,759, 3,062—that we may consider ourselves possessed of a very fair approximation to their real magnitudes.

As to the "albedo" of their surfaces, I. shows no great variation; it falls, according to Zöllner's estimate of the reflective power of terrestrial materials, between that of marl and white sandstone; II. has the greatest variations of albedo, which at a mean somewhat exceeds that of white sandstone; III., the variations of which are smaller and more regular, comes between marl and quartzose porphyry; IV., which varies least, equals that of moist arable land. It will probably be thought, however, that curious as these comparisons may be, the standards are much too uncertain to give any satisfactory result. As to colour, Dr. Engelmann, after citing the elder Herschel's estimates—I., white; II., white, bluish, and ash-coloured; III., white; IV., dusky, dingy, inclining to orange, reddish, and ruddy—specifies as the determination of other observers: I., yellowish; II., white or yellowish; III., intensely yellow with low powers; IV., in achromatics a distinct dusky blue. (These colour-values at any rate afford no countenance to the common impression that Herschel had a bias for red tints.) To the writer, whether with two achromatics, or a nine-inch silvered mirror, this satellite has always appeared ruddy when its colour has formed the object of notice; in such discrepancies something may be instrumental, something subjective. It is pleasant to see here a very full appreciation of the laborious perseverance and honest accuracy of the labours of Schröter, to whose merit time seems to be doing tardy justice; no notice is taken, however, of the observations of Gruithuisen, who twice appears to have seen spots on III on the background of the sky; nor is reference made to the irregular shape of that satellite remarked by Secchi and his assistant; nor to the apparent discrepancy which has often been noticed between the magnitudes of the satellites and their shadows. Still, the treatise may be considered as very nearly an exhaustive one; and a most important and acceptable contribution to planetary astronomy. It may be added that it contains a very valuable determination of the telescopic magnitude of Jupiter, from the average of eleven observers; the result being, with the double-image micrometer 37″·609 for the equatorial, 35″·236 for the polar diameter; with the wire-micrometer, 38″·312 and 35″·914: the former values, which he seems to prefer, exhibiting a flattening of $\frac{1}{15\cdot82}$.

T. W. WEBB

OUR BOOK SHELF

Transactions of the Geological Society of Glasgow. Vol. III. Supplement. On the Carboniferous Fossils of the West of Scotland: their Vertical Range and Distribution. By John Young, Vice-President. With a General Catalogue of the Fossils and their Mode of Occurrence, and an Index to the Principal Localities. By James Armstrong, Honorary Secretary. (Glasgow, 1871.)

THIS catalogue of fossils will doubtless be of great use not only to local geologists, but to others at a distance, who may desire to compare the treasures of English and Irish Carboniferous strata with what the equivalent beds in Scotland have yielded. So far as they go, the lists appear to be drawn up with considerable care, and Mr. Armstrong is to be congratulated upon the result of what must have been somewhat laborious work. But we are sure he will be the first to admit that much, very much, still remains to be done before the Scottish Carboniferous flora and fauna can be satisfactorily compared with those of other countries. We are constantly being reminded throughout this catalogue that not only in private collections, but also in public museums in the West of Scotland, there are numbers of specimens under almost every class waiting to be identified, amongst which there is every reason to believe that not a few are species new to science. This, it seems, is specially the case with the plants, the rich flora of the Carboniferous period being represented in the catalogue by only ninety species. But Mr. Carruthers, we are told, has several undescribed specimens in hand, of which we shall, no doubt, hear by-and-by. The fishes, it would appear, also need looking after. There are eighty-four species, under forty genera, named in the catalogue; but a large number in various collections have never been correctly identified with described species, and Mr. Young expresses a hope, in which we cordially join, that Prof. Young will be induced to prepare a special catalogue of these and the Reptilia, of which only seven species are given by Mr. Armstrong. The other classes are represented as follows:—Foraminifera, 2 genera, 4 species; Hydrozoa, 1 g. 2 sp.; Zoophyta, 22 g. 59 sp.; Echinodermata, 6 g. 15 sp.; Annelida, 4 g. 7 sp.; Crustacea, 19 g. 71 sp.; Insecta, 2 g. 2 sp.: Polyzoa, 11 g. 36 sp.; Brachiopoda, 15 g. 50 sp.; Lamellibranchiata, 28 g. 127 sp.; Pteropoda, 1 g. 1 sp.; Gasteropoda, 15 g. 75 sp.; Cephalopoda, 6 g. 46 sp. From these numbers it will be seen that the collectors have not been idle, and, no doubt, Mr. Armstrong's catalogue, with its minute index to localities, will be the means of sending many to hunt in quarters which they have not already visited. Let us hope that they will note something of the conditions under which the fossils are distributed, and not content themselves simply by bringing away good bags full. Collectors cannot be too often reminded that it is of more importance, in the interests both of natural history and geology, to know one limited district thoroughly, than to go roving over half a country merely for the purpose of picking up finely preserved specimens. Each should mark out for himself some practicable area, and make it his endeavour to search every bed, even the most unpromising, noting not only the fossils he meets with, but the character of the strata in which they occur. He should also observe what effect a change in the character of a bed has upon the fossils it may happen to contain; whether they increase or decrease in numbers, whether they individually gain in size or become dwarfed, and, should certain species disappear, what others, if any, are substituted for them. It is only by marking carefully such points as these that we can ever hope to acquire an adequate conception of the natural history of the old carboniferous lands and seas. Mr. Young is quite sensible of the shortcomings of the collectors in this matter, and gives them some seasonable advice, which it may be hoped they will take to heart. If collectors paid better heed to these

matters they would assuredly derive greater pleasure and profit from their pursuit, and do much more towards the progress of science. Mr. Young himself, however, notwithstanding the good advice he gives, is not always careful in drawing conclusions, geological evidence being sometimes quite overlooked. Thus, we find him stating that the coal-measures (meaning, of course, the whole series of strata above the Millstone Grit) are "evidently of land and fresh-water origin," because they have yielded no marine organisms, save in one thin local bed near the top of the series. The occurrence of this stratum with its marine remains, indicates, as he believes, the return for a short time of the sea, which had for a very long period "been completely shut out by barriers." Mr. Young is welcome to his belief. If every bed or series of beds in which no marine organisms occur must necessarily be of fresh-water origin, the lakes of old must have been something worth seeing. There are several points suggested by the catalogue that we should like to have taken up, but our space is exhausted, and we can only conclude by strongly recommending Mr. Armstrong's work to the notice of our geological readers.

J. G.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

On the Solution of a Certain Geometrical Problem

A WRITER in the number of NATURE for September 21, Mr. R. A. Proctor, in the course of a letter on the state of geometrical knowledge in the university, alludes manifestly to the solution of a problem which I have adopted in my edition of Euclid. The matter is of small importance in itself, but nevertheless as some points of interest are incidentally involved, I request you to allow me the opportunity of offering a few remarks.

The problem is this: to describe a circle which shall pass through a given point and touch two given straight lines. Your correspondent considers that in giving a solution which depends on the sixth book of Euclid, instead of one which depends only on the third book, I exhibit signal geometrical weakness.

The problem, I need scarcely say, is very old; indeed, so old that a writer who had been long engaged in teaching could not pretend to solve it afresh, for he would certainly have in his memory one or more solutions which had become quite familiar to him. The solution by the aid of the third book is well known, for it occurs in several of the collections of geometrical exercises. The solution which I have adopted is also old, but seems not so well known. It is, I think, conspicuous for simplicity, elegance, and completeness. The demonstration is of the best and most impressive kind, requiring no laborious effort to understand and retain it, but being almost self-evident from the diagram. Even if the problem be treated as an isolated exercise, the solution which I have preferred will sustain a favourable comparison with that which more commonly occurs.

But the determining cause of my choice was the position which the solution occupies as one of a connected series. I have just before treated a similar problem by the third-book method, so that if the same method had been used for the present problem, there would have been only repetition without any substantial increase of knowledge; whereas by the course adopted the student is introduced to fresh and valuable matter. The principle of similarity and the notion of a centre of similitude are most instructively involved, and the student is prepared for a subsequent investigation, which is similar but more complex. To sum up, the third-book method would have constituted no advance in the subject, where the sixth-book method takes a step important in itself and in its consequences; and therefore, following the example of an eminent geometer, I adopted the latter method. I may perhaps venture on the strength of my own experience as to the utility of the solution, to recommend it to the attention of other teachers.

It is very important to bear in mind the distinction between what I may call absolute and relative merit which I have just exemplified. The solution of a single problem furnished by a candidate under examination, or by a contributor to a mathematical periodical, is very different from the investigation of one out of a chain of propositions in a mathematical treatise. In the former case there are no antecedent or subsequent conditions to regard; in the latter case we have to consider what agrees best with the whole scope of the work, with what is to follow as well as with what has gone before. A writer, after arranging a paragraph or a chapter in what seems the best manner, may find himself constrained at a subsequent stage to make changes which would have been unnecessary, perhaps even undesirable, if the earlier portion had stood alone. Then, if a reader opens the book at random and criticises a passage without any regard to the author's sense, the criticism may very naturally be quite inappropriate.

There is, however, a very important consideration of another kind which has been frequently disregarded, but which is pressed upon our notice by the interest at present felt in geometrical studies. Let us determine the reason which leads us in some, or in many, cases, to prefer a solution which involves only the third book of Euclid to a solution which depends on the sixth book; this, I apprehend, is merely a persuasion that Euclid's order is a natural order, so that in a well-arranged system the propositions of the third book ought to precede those of the sixth book. I am of this persuasion myself; I think that no scheme can be perfect, and, on the whole, I am well satisfied with Euclid's. But there are places where Euclid is strong, and there are places where Euclid is weak; and the position which he has assigned to the last three propositions of his third book, must rather be classed with the latter than with the former. His object, of course, must have been to lead up to his construction of a regular pentagon, and we cannot be surprised at the introduction of that remarkable process. But I have always envied the advantage in this respect to be claimed for the non-Euclidean systems, which transfer these propositions and place them after the doctrine of similar triangles; thus the long and rather artificial treatment which they receive from Euclid is superseded, and the propositions become almost intuitive. Hence, in fact, if we have recourse to the sixth book of Euclid when we might have accomplished our end by the aid of the first thirty-four propositions of the third book, we may be fairly liable to the charge that we have not adopted the simplest and most natural method; but the last three propositions of the third book are quite different in kind from the others, and instead of using them, it may be really as simple and as natural in many cases to use the principle of similar triangles.

I shall be obliged to any person who may be skilled in practical geometry if he will state what he considers the best method of *actually* solving the problem, supposing that both circles are to be determined which satisfy the conditions. I assume that we have the aid of compasses and also of one of the ordinary contrivances for drawing parallel lines. This is a matter of some interest, though of course unconnected with the theoretical solution of the problem.

I should be glad to make some remarks on the general subject which led to the notice of the particular problem I have discussed, but at present I have not sufficient leisure. I must content myself with having shown that the course into which I am supposed to have drifted by geometrical incapacity, was adopted deliberately under the guidance of reasonable geometrical knowledge.

I. TODHUNTER

St. John's College, Cambridge, Oct. 2

Structure of Fossil Cryptogams

IT was unfortunate that at the recent meeting of the British Association, Prof. Williamson's paper had to be discussed in a very hurried manner, and he is, no doubt, justified in taking care "that there shall be no misunderstanding as to the real point at issue." I do not think that he has brought it out very plainly in his paper in NATURE, and perhaps, as he mentions me as an opponent of his views, I may be allowed to state precisely in what respects I differ from him.

First, as to matters of fact. Prof. Williamson speaks of the central structure of the stems of the extinct Lycopodiaceæ as a "vascular medulla," by which he explains that he means a "structure containing vessels," and that there shall be no misapprehension he adduces *Nepenthes* as possessing it; the instance is a well-known one, and leaves no room for doubt as to Prof. Williamson's meaning. Now from the examination of specimens, and of the drawings of them published by Mr. Carruthers (the accuracy of which I believe Prof. Williamson does not dispute) I am quite satisfied that the central structure consists wholly of

scalariform vessels, and that there is in fact nothing medullary or medulla-like about it.

Outside this central structure is what Mr. Carruthers terms the investing, and Prof. Williamson the vascular woody cylinder. I believe that Mr. Carruthers is right in looking upon this as belonging to the central axis, which is therefore composed of two parts.* I find, which I did not sufficiently appreciate at the time, that Prof. McNab regards this investing cylinder as homologous with the cylinder of wood cells surrounding the central axis of fibro-vascular bundles which is met with in many recent Lycopodiaceæ. From this I certainly dissent for two reasons; (1) because I think its equivalent is to be found in the central axis itself, and not outside it; (2) because it is not composed of wood cells but of scalariform vessels.

Secondly, as to opinions. The terms Exogen and Endogen, as is pretty well known, were founded upon a mistake. A great deal too much has been made of the difference implied by them; in fact, if we compare a one-year-old dicotyledonous shoot with a monocotyledonous stem, we find that it does not exist. If Prof. Williamson will look at the stem of the common artichoke, he will find it difficult to convince himself that he is examining an "exogenous" plant at all.

The imagined characters which were implied by these terms are, nevertheless, as everyone knows, correlated with others, which in the aggregate enable phanerogamic plants to be divided into two satisfactory groups; but this is certainly not equally the case with the groups into which Prof. Williamson would divide the vascular cryptogams. These groups, I think, most botanists will agree in considering in the highest degree unnatural, inasmuch as, assuming the vegetative distinction upon which they are founded to exist, it is a wholly artificial ground for classificatory purposes. Nor is it any argument that one vegetative character must be good because others are in use, since the simple answer is that these coincide with natural divisions, while Prof. Williamson's does not.

I shall not dispute Prof. Williamson's position that our living Lycopodiaceæ should be interpreted by the more complete extinct types. To do this, however, the extinct types must be thoroughly understood; when we are dealing with imperfect material, comparison with the more perfect but less highly developed existing plants is not only justifiable but necessary.

It is obvious that the great development of the stem in the Lycopodiaceæ of the Coal Measures was correlated with their arborescent habit. I am inclined to think with Prof. Williamson that the stem increased in thickness; it is certain that *Lepidodendron* was branched, and not improbably also *Sigillaria*. The branches as they were gradually developed must have been the cause of an increasing strain upon the stem; it seems to me more congruous with known laws of the response of structure to circumstances, to conclude that the stem was proportionately developed as the strain increased, than that the stem should have been produced once for all of its maximum thickness without reference to the crown of branches that was finally to surmount it.

I am quite prepared therefore to admit that the investing cylinder may have increased by external additions, and probably did do so; this would of course imply the existence of a cambium layer outside it. There is some analogy for this in the recent *Isoëtes*, where we have a "slight woody mass which occupies the longitudinal axis of the stem, but encloses *no pith*." † Outside this we have a "bark-forming" cambium (which also adds, but more sparingly, to the wood mass); in *Sigillaria* and *Lepidodendron* we might have had a cambium not merely renewing the bark but adding to the central axis.

In whatever way the increase took place, it was, as I think, nothing more than an incident in the life history of a particular race of plants, nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost, but showing a lingering ancestral tendency in *Isoëtes*. Comparing a simple stemmed palm with *Dracæna*, we have a parallel instance of the strengthening of the stem *pari passu* with the continued development of a system of branches; only in *Dracæna* it is the circumferential part of the stem alone which developes.

If I am right in regarding a stem gradually developing in size as the necessary correlate of a large system of branches, Prof. Williamson's view practically amounts to the old division of plants into trees and herbs. I cannot see how it can afford any safe ground for a re-arrangement of the vascular cryptogams.

W. T. THISELTON DYER

London, Sept. 26

* *Monthly Micro. Journ.*, 1869, p. 169.
† Hofmeister, Higher Cryptogamia, pp. 356, 361

The Solar Spectrum

MAY I venture to suggest that quite possibly something of value might be obtained by observing the sun during totality with a spectroscope of reasonable dispersive power (say four or five prisms) *without a collimator*, or even simply with one of the so-called meteor spectroscopes.

If the bright rays and rifts are really and simply (or even mainly) composed of the green-line-giving substance, they will give a well-defined green image; if they are formed by reflection (either at the sun or in our atmosphere) of ordinary sunlight, they would be so dispersed as to be invisible or nearly so, and if formed by the reflection of chromosphere light they would give several images, the red (C) and blue-green (F) being most conspicuous.

C. A. YOUNG

Hanover, N.H., U.S., Sept. 13

*** Arrangements have already been made for carrying out a similar suggestion to this by the Eclipse Committee; and the corona will also be observed with an open slit.—ED. N.

Eclipse Photography and the Spectroscope

THE endeavour of the Eclipse Committee to secure some uniformity in the photographs from different stations next December does not appear to be duly appreciated, it being contended that immense "personality" shown in various photographers' manipulation must frustrate the good intention. I submit that in this case the personality is greatly over-estimated; that a number of competent photographers taking the same subject would probably produce, under any ordinary circumstances, pictures bearing considerable resemblance; while by using like apparatus and giving exposure of the same duration, we might safely predict a similarity of result amply sufficient for comparative purposes, and for the identification of structural peculiarity should it exist.

Among others there is a possible advantage to accrue from uniform work by the philosophers which I have not seen or heard noticed. Supposing the outer corona, rays, streamers, or any portion of the apparently luminous matter be terrestrial, is it unreasonable to expect that photographs, taken at stations more or less widely separated, will, when properly combined in the stereoscope, give clear ocular proof of the sublunary situation of such luminous matter?

HENRY DAVIS

Phenomena of Contact

MR. STONE can safely be left to meet the arguments specially addressed to him in Prof. Newcomb's letter; but as the subject relates to the only point of importance touched on in Prof. Newcomb's criticism of my chapter on the sun's distance, I crave permission to meet his general argument.

I submit that he tries to prove too much.

He admits that the phenomenon of irradiation exists in the case of a disc. The sun's disc, then, must be to some extent enlarged, and the dark disc of Venus must be to some extent reduced by the effects of irradiation. Now this being so, *what becomes of the cusps*, when Venus is all but wholly on the sun's disc? Either the irradiation is diminished near the cusps or it is not. If it is diminished there must be distortion, because the disc of Venus is then not uniformly reduced: if the irradiation is not diminished a ligament must appear.

Let any one draw a large circle (say a foot in diameter) on paper, and a small one (say an inch in diameter) extending very slightly (say by the twentieth of an inch) beyond the boundary of the first; and let him blacken the smaller circle as well as all the space outside the larger one. He has then a space representing the disc of the sun with a very large Venus upon it near the time of internal contact. Now let him conceive the whole of this space (a sort of exaggerated crescent) slightly enlarged as by irradiation, the enlargement-fringe extending outside the boundary of the large disc and inside the boundary of the small black (incomplete) disc. He will find the conception of this enlargement exceedingly easy everywhere save near the cusps; but here there is a difficulty in determining how the fringe outside the larger disc is to be joined on to the fringe inside the smaller disc. If he can conceive these two fringes meeting in such sort as to leave the reduced outline of the small disc completely circular up to the very points in which it meets the enlarged outline of the large disc, he will have done what Prof. Newcomb's theory requires. But note, this must be done for the case when the fringe of enlargement is wider than the twentieth of an inch, by which the small disc overlaps the large one. When this is the

case, the task will be found to be impracticable; but even when the overlap of the small disc is greater, the task can only be achieved by actually making new cusps out of the irradiation fringes. (A figure would make this explanation much simpler.)

Prof. Newcomb says that he is decidedly of opinion that the irradiation of an extremely minute thread of light is not the same with that of a large disc. He does not seem to notice that if this is so, Venus just before, at, and just after internal contact, *must* be distorted. This even if—admitting the enlargement of the sun's disc—he denies that the disc of Venus is reduced by irradiation.

He fails also to observe that a peculiarity such as distortion, or the formation of a ligament, may escape the notice of inferior or not very attentive observers, and so all his negative observations be explained. It is no proof of superior skill in observation to see no signs of an illusory effect. Until we have observers who recognise no traces of irradiation when looking at the solar disc, we must believe that (as Mr. Stone has, I think, already asserted) the non-recognition of distortion or ligament formation is due to inattention, or want of observing skill. That this should be more common than close and careful scrutiny is not a very surprising circumstance, and proves nothing. RICHARD A. PROCTOR

Oceanic Circulation

IN NATURE of August 17, I have just seen the report of the discussion on Dr. Carpenter's paper on the above subject read at the late meeting of the British Association.

Dr. Carpenter, explaining the movements on thermodynamic principles, states that he has "found the *primum mobile* of this circulation was not in equatorial heat but polar cold," and explains that "(1) As each surface-film cools and sinks, its place will be supplied, not from below, but by a surface influx of the water around; and (2) the bottom stratum will flow away over the deepest parts of the basin, while, since the total heat of the liquid is kept up, there will be an upper stratum which will be drawn towards the cold area, to be precipitated to the bottom and repeat the action. Apply this principle to the great oceanic area that stretches between the equator and the poles, we should expect to find the upper stratum moving from the equator towards the poles, and its lower stratum from the poles towards the Equator. That such a movement really takes place is indicated, as it seems to me, by various facts."

It does not appear, however, that Dr. Carpenter has well established his claim to the theories in question, while, in a pamphlet on the same subject, published in 1869 by Dr. Adolph Mühry of Göttingen, we find such passages as the following:—"As the cause of the latitudinal circulation we have assumed the difference of temperature in the water between the equator and the pole." He honestly gives Arago the credit of being, perhaps, the first to put forward this view in 1836; and after remarking (p. 11) that it might be considered doubtful whether it is the upper warm current from the equator or the under cold one from the pole that ought to be considered the primary, he says (p. 12) "For us the primary 'arm' is the heavier, *i.e.*, the colder polar stream, which, in obedience to gravitation, falls in a horizontal direction toward the lighter water of the hot zone; and the secondary 'arm' is the returning antipolar. It moves to replace what flows away, and is, therefore, the compensation-arm."

Here, without following Dr. Mühry any further, we find the thermodynamic theory advanced by Dr. Carpenter, and his *primum mobile* as well; but by giving him credit for ignorance of Dr. Mühry's work, we may excuse him for laying claim to what is there put forward, and accepting therefore the commendation of others as unknowing as himself. J. B.

Ice Fleas

DURING a recent ramble upon the Morteratsch Glacier, I also observed a large number of the minute black creatures described by Prof. Frankland in NATURE, No. 100. My attention had been directed to them ten years ago by Lord Anson on the "snow-bones," near the summit of the Ægischorn. They are only nominal "cousins" of the flea (*Pulex*) of civilised life, and are not at all related to *Dapnia*, the "water flea," but are closely allied to the minute insects which are often seen on the surface of stagnant water, resembling grains of gunpowder, and skipping partly by help of their forked tail, folded under them so as to serve as a foot, hence their name *Podura*, or "skip-tail." They have been named by Agassiz *Desoria saltans*. Their food, I conjectured with Prof. Frankland, consists of "red snow" and other microscopic algæ. Not being myself within reach of a good library, I can only furnish your readers with a key to further information. C. A. JOHNS

IN NATURE of 28th September, Prof. Frankland, in introducing the ice flea to the readers of NATURE, uses the expression "if known at all," and concludes by asking information about it. The glacier flea, *Desoria glacialis*, was noticed and described by Prof. Agassiz as far back as 1845, in his Ascent of the Wetterhorn on the 29th of July of that year. Not having Agassiz's work at present beside me, I cannot refer to it, but these fleas are noticed in an extract translated from an account of the ascent, and published in *Hogg's Weekly Instructor* for Dec. 1845, vol. ii. p. 221. On the Aar Glacier they are described as being scattered over the "surface of the snow in millions," elsewhere, "as being collected in masses under the stones on the ice." R. C.

The New Dynameter

THE letter from the Rev. T. W. Webb in your last number is a very tantalising letter. He tells us, and we could not wish to have a better authority, that a new dynameter has been invented by the Rev. E. Berthon, but he does not tell us how it is constructed or where it can be obtained.

I may take this opportunity of mentioning a makeshift dynameter which I have found to answer very well when extreme accuracy is not required.

I have a pocket telescope fitted with a Cavallo micrometer, *i.e.*, a slip of finely divided mother-of-pearl screwed to the diaphragm next the eye-glass. Unscrewing the two last draws of this telescope the end of the second is applied to the eye-piece of the telescope of which the power is to be measured, and the first draw pushed in till the image of the object-glass comes sharp upon the mother-of-pearl. The diameter of the image is thus given in divisions on the mother-of-pearl, the value of which, in hundredths of an inch, has been previously ascertained. W. R.

Notaris on Mosses

WITH reference to the notice of De Notaris' book on Mosses, I am informed by Dr. Dickie that the genus *Habrodon* was discovered in Great Britain several years ago by the late Mr. McKinlay, of Glasgow, and that he had received from Mr. Wilson about two years ago from his district *Conomitrium julianum*. Dr. Dickie sends specimens of *Habrodon Notarisii* gathered at Killin by Dr. Stirton. M. J. BERKELEY

*** In the review referred to, Prof. De Notaris was erroneously described as of Geneva, instead of Genoa.—ED. N.

"Newspaper Science"

MY attention has just been called to a letter from Mr. David Forbes which appears in NATURE, Sept. 21, under the head "Newspaper Science," and in which that gentleman, writing from Boulogne, pathetically describes the emotions with which he read a recent "*article*" in the *Globe* on "Krupp's" Gun-manufactory at Essen. I need hardly say how deeply I deplore the shock which I have unwittingly been the agent of inflicting on your distinguished correspondent. It will be some small satisfaction if you will allow me to express the hope that the "desired result" of Mr. Forbes's "reluctant" compliance with the advice of his "medical man," and most wise resolve "to eschew everything scientific for the next few weeks at least, in order to recruit before the winter labours commenced," may not be utterly defeated by the perusal of "a specimen of English scientific opinion," of which I am unhappily the author. It would be a terrible reflection indeed, that a stupid error on my part had, perhaps, imperilled the accuracy and success of Mr. Forbes's "winter labours." The blunder (or rather blunders) occurred as follows:—I, too, was "knocked up with work," but being myself a "medical man" naturally only in part carried out my own prescription. I would, for the sake of Mr. Forbes, and the credit of "English scientific opinion" in the estimation of his "French acquaintance," I had exercised a little more discretion. However, unfortunately, I stumbled on the Krupp factory, and all forgetful of my dilapidated mental condition, wrote a note-paragraph (I never write "articles"), which I vainly imagined might have been innocent and interesting. It is not always possible to compress even the manuscript necessary for a paragraph on to a single sheet of paper, and I grieve to say that after my paper had passed the editorial eye three words

forming the connecting link of a sentence must have been dropped. What I intended to say, without the slightest notion of giving a "technical or scientific" opinion, was, "The iron is alloyed *in crucibles, formed* with certain clays and a preparation of plumbago." The words italicised disappeared in some mysterious way. The next of my idiotic sentences goes on to talk about the crucibles, or "*creusets*," as, to the great scandal of Mr. Forbes, I ventured to call them. If I could stop here, an humble apology for my fault might, perhaps, serve my purpose, but, alas! I have more to answer for. Vaguely dreaming of the foot-pound, I actually *wrote* kilometre for kilo*gram*metre, when speaking of the power of the new steam hammer; and, worst of all, I also WROTE "Sheffield Gun Metal."

Can I ever hope to be forgiven when thus I write myself down an ass? MEDICUS

P.S.—As to the question whether Krupp invented the process employed at his factory, I offer no opinion "scientific" or ordinary. I only repeat the impression which prevails.

FURTHER NOTES ON CERATODUS

SINCE the article on *Ceratodus* (published in NATURE, Nos. 99 and 100), was written I have examined a mature female, transmitted, with other examples, by the Trustees of the Sydney Museum to the National Collection, and am enabled to make the following additions:—

1. The oviduct in its developed conditions is, with regard to its internal structure, surprisingly similar to that of *Menopoma*.

2. The ova are expelled through the oviduct, and not through the peritoneal slits; they receive in the oviduct a coating of an albuminous substance as in Batrachians.

3. The caudal termination of the vertebral column is subject to individual variation. In one example the neural and hæmal elements are continued far beyond the notochord, and are confluent into a tapering band, which is segmented, as is the case in some specimens of *Dipterus* or *Ctenodus*. ALBERT GÜNTHER

ON THE BENDING OF GLACIER ICE *

MR. MATTHEWS and Mr. Froude had supported long rectangles of ordinary ice at the two ends, weighted them in the centre, and thus caused them to bend. The ice employed, if I recollect right, was of a temperature some degrees below the freezing point, and in my little Alpine book recently published I expressed a hope that similar experiments might be made with glacier ice. I have been trying my hand at such experiments. The ice first employed was from the end of the Morteratsch Glacier, and when cut appeared clear and continuous. A little exposure, however, showed it to be disintegrated, being composed of those curious jointed polyhedra into which glacier ice generally resolves itself when yielding to warmth. Still, when properly supported and weighted, a long stout rectangle of such ice showed, after twelve hours, signs of bending.

I afterwards resorted to the ice of the sand cones, which, as you know, is unusually firm. From it rectangles were taken from three to four feet long, about six inches wide, and four inches deep. Supported and weighted for a considerable time, no satisfactory evidence of bending appeared; the bars broke before any decided bending took place. Smaller bars were then employed. Two of these were placed across the mouth of an open square box, their ends being supported by the sides of the box. They formed a cross, and a clear interval of at least an eighth of an inch existed between them where they crossed. The upper one was carefully weighted with a block of ice; after two hours it had sunk down, and was found frozen to the under one. They were then separated, and one of them was allowed to remain supported at the ends and weighted by ice at the middle. In a few hours it had bent into a curve, the versed sine of which from a chord uniting the two ends was, at least, two inches. In fact, *when the rectangles are thin*, and the weight carefully laid on, flexure commences very soon, and may by cautious manipulation be rendered very considerable. I think Mr. Froude told me that in his experiment the molecules were "in torture," and that they in great part recovered their positions when the weight was removed. In the foregoing experiments the flexure was *permanent*.

I tried to bend the rectangle just referred to back again by reversing its position and weighting it with the same block of ice. But whether owing to my want of delicacy in putting on the weight, or through the intrinsic brittleness of the substance itself, it snapped sharply asunder.

I left in your hands when quitting London an exceedingly interesting paper by Prof. Bianconi, in which are figured the results of various experiments on the bending of, I think, lake ice. The foregoing experiments on glacier ice confirm his results.

August 4 JOHN TYNDALL

I may add that various experiments were subsequently made, and a means discovered of rendering the bending very speedily visible. I hope before long to return to the subject.—J. T., September 28

* The following is an extract from a note addressed to Prof. Hirst, and sent from Pontresina in the hope that it would reach Edinburgh in sufficient time to be communicated to Section A of the British Association. It was a few hours too late.—J. T.

THE MIGRATION OF QUAIL

THE fact of this little bird having visited England this year in such numbers appears to have attracted the attention of naturalists as well as sportsmen. In the columns of the *Field* may be found a census giving particulars of this migration. And it will appear a curious coincidence when I mention that there has been here a greater migration of quail this year than ever remembered before. Where they come from is somewhat mysterious. They have been shot in hundreds in some paddocks, and found as numerous as ever in ten days. I can only account for it by stating that it has been a most remarkable year for grass, and consequently cover was good; and this does not appear conclusive, for the grass has been good all over the country for hundreds of miles towards the north, from which direction some appear to think they come. They are found generally in paddocks, where thistles grow. Can there be any common cause affecting these facts?

Melbourne, August 10 AUSTRAL-ALPINE

JARDIN D'ESSAI, ALGER

IN 1832 the then French Government conceived the idea of forming near the town of Algiers a botanical garden, in which all plants likely to be easily grown in Algeria, and which might be useful either for their ornamentation, or from their economic value, should be kept for distribution or for sale. A portion of ground situated between the sea and the public road, and occupying the place of an old *hamma* or marsh, was selected for this purpose, which is about two miles from the town. In 1867 the Emperor of the French conceded this establishment to the "Société Générale Algérienne," under whose auspices, but under the direct superintendence of M. Auguste Rivière, the gardens at present are.

In addition to the level swamp, the gardens now also occupy the slope of a low hill on the opposite side of the road. The level ground is laid out in alleys which open out into a circular boulevard which surrounds the whole garden. Carriages are admitted to the circular drive only, foot passengers to the cross walks. A stream of fresh water runs through the grounds, forming in one place a small lake.

One fresh from the Botanical Gardens of Europe is astonished at every step taken in the Gardens by the wondrous vegetation which is shown by all the semi-tropical plants. Descending a few steps from the circular drive, a great palm avenue is entered. This avenue was planted in 1847, and is formed of about eighty trees of the date palm, nearly as many of the *Latania Borbonica*, and about 150 of the dragon's blood tree (*Dracæna draco*). The avenue is about ten yards wide, and between every two of the date palms there are two of the dragon's blood tree and one Latania. It terminates in a clump of palm trees which are planted almost to the border of the sea. When it is borne in mind that the date palms are from twenty to fifty feet high, the Latanias averaging about twelve, and the Dracænas about eight feet in height, the general effect of this splendid avenue may be imagined. All the trees were in December last in full flower or fruit, the golden trusses of the date palm contrasting well with the more brightly-coloured clusters of Latania berries. It would require more space than is at our disposal to describe the contents of all the various small avenues that branch off from the main one. The most remarkable smaller avenues are, perhaps, the one formed of bamboo (*Bambusa arundinacea*), planted in 1863, and forming an immense mass of foliage, the stems supporting which are from forty to fifty feet high, and that formed of about 100 plants of *Chamærops excelsa*, each about ten feet in height. But remarkable as are these charming sub-tropical alleys, the visitor is more than surprised when on going towards the portion of the garden where the plants are grouped somewhat according to their natural orders, he finds specimens fifteen feet high of *Caryota urens* and *C. Cumingii*, growing with vigour and covered with fruit; of *Oreodoxa regia*, from Cuba; several plants upwards of twenty-five feet in height; and a plant of *Jubæa spectabilis*, which is twelve feet high; and then just a few steps more and a parterre alloted to the natural family of the Musaceæ comes to view. As both the plantain and banana are grown in large quantities for their fruit in another portion of the grounds, the family is here chiefly represented by such genera as *Strelitzia* and *Ravenalia*. Magnificent specimens of the latter genus, with stems nine to ten feet high, exhibited great combs of flowers. We are not aware if the Traveller's tree has flowered in Europe, and we were not prepared to find it in full flower in Algiers. It has not, however, matured its fruit in this garden. Near this grand parterre stood another with many fine specimens of Yucca, also a magnificent plot of Aralias, *A. papyrifera*, in full fruit and very handsome; the fine *A. leptophylla* and *A. præmorsa*, thickly covered with spines, and the very ornamental *A. farinifera;* and then one's attention is caught by a large tree (*Carolinea macrocarpa*) from Brazil, with a couple of dozen of its fruit, each as big as a cocoa nut; by a small forest of *Anona cherimolia* in full fruit, which is nearly as good as that of the closely related species which yields the custard apple. Near these is an immense tree some thirty feet in height, covered with fruit of the Avocado pear (*Persea gratissima*); and at its feet is a quantity of guava trees (*Pisidium Cattleyanum*) crowded with its perfectly ripe, large, pear-shaped, golden fruit. Growing up into the trees, and forming numerous and never-ending festoons, were some specimens of Cacti, chiefly species of Cereus. Some of these were of great size, and one specimen, which had completely strangled a plantain tree some twenty-five feet, was said to have been covered in the autumn with 600 to 700 flowers. It must have been a sight worth a long pilgrimage to see.

Enough has been said to show what a surprising number of semi-tropical fruits luxuriate in the beds of this well-watered garden, and we might add many well-known vegetables to the list, as sweet batat, yam, papaw; but all this while we have been writing of the great level portion of the garden. Outside of this, and on the other side of the roadway, there is a small hill, two or three hundred feet in height, which slopes towards the garden and the sea, and is traversed by several ascending walks. This is the New Holland district of the garden, and certainly not the least interesting portion of it. In one section of it are different species of Acacia, many of them large trees, twenty to twenty-five feet in height. Of the Proteaceæ there were magnificent trees; of the genera *Banksia*, *Hakea*, and *Grevillea*, the collection of species was very large, all of them just bursting into masses of bloom. The most important of the trees growing in this corner of the hill was probably *Eucalyptus globulus*, of which some trees, now about forty feet in height and over four feet and a half in circumference, were planted in 1862, and were then only a few inches high. Young well-established seedlings, of about ten inches in height, are sold for 20*s*. a hundred, and large numbers of them have been planted from time to time throughout Algeria by the French Government. This species grows in Algeria with most surprising rapidity, under very favourable circumstances growing eighteen to nineteen inches in height each month. Its wood appears to be hard, close in the grain, and it is largely used in the construction of quays, bridges, and railways. This tree seems to do so well on the southern side of the Mediterranean that we think its culture ought to be successfully attempted in the south of Spain, in Sardinia, in Sicily, and the southern parts of Italy. In districts subject to heavy winds it requires for some years—owing to its rapid growth—some protection, but in places sufficiently warm for it, it ought to repay well for any little extra care it might be found to need.

Among the few species that we noticed that did not succeed in these gardens, we may mention the *Cedrus deodara;* but *Casuarina equisetifolia* was flourishing, and one tree of *Araucaria excelsa* was about sixty feet in height, and measuring a little over nine feet in circumference at its base.

The object of the Society in keeping up these Gardens is, as we said, to introduce into Algeria all useful and ornamental plants likely to grow there. In addition they grow enormous quantities of young palms and other ornamental plants for exportation to Europe, and some few plants interesting to the botanist for exchange with other establishments. In a place so favoured by nature and so easily accessible to Europe, it would be, we venture to think, well worth the while of the director of these Gardens to considerably enlarge the last portion of the Society's design. How many tropical plants are yet unknown to the large collectors of Europe, and what a vast percentage of deaths occur among the collections sent from the tropics at any season of the year to our shores! But with Gardens like these at Algeria, situated on the sunny side of the Mediterranean, to act as a half-way house, the resources of the Botanical Gardens or establishments of the North would be indefinitely increased. Another purpose for which these Gardens might be made most useful is for forming a collection of specimens of plants or fruits of economic interest. Many of the fruits, stems, &c., which ripen in these Gardens as easily as cherries or potatoes with us, are not to be seen in some botanical collections, and are not, in Europe at least, to be purchased. How gladly would some botanist buy such as we here refer to if they were on sale, say at the depôt of the Algerian Society in Paris; and the expense of putting up such in salt and water would be a mere nothing. The same remarks would apply in many cases to portions of the roots of remarkable genera, and also to flowers. In calling attention to these Gardens, we venture to suggest these hints to their well-known director, and also to that indefatigable botanist who, more than any other, now represents science in connection with the Algerian Society, Prof. Durando of Algiers.

E. P. W.

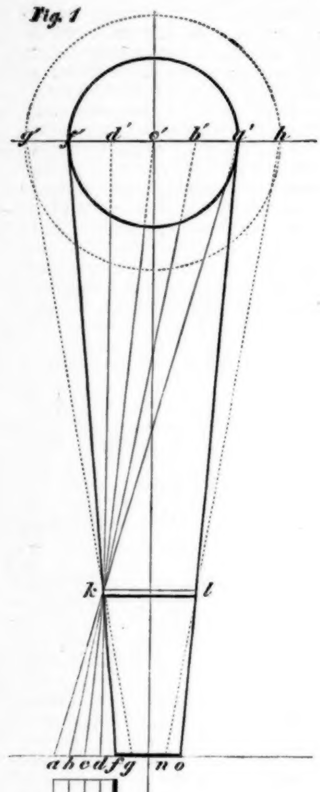
THE TEMPERATURE OF THE SUN

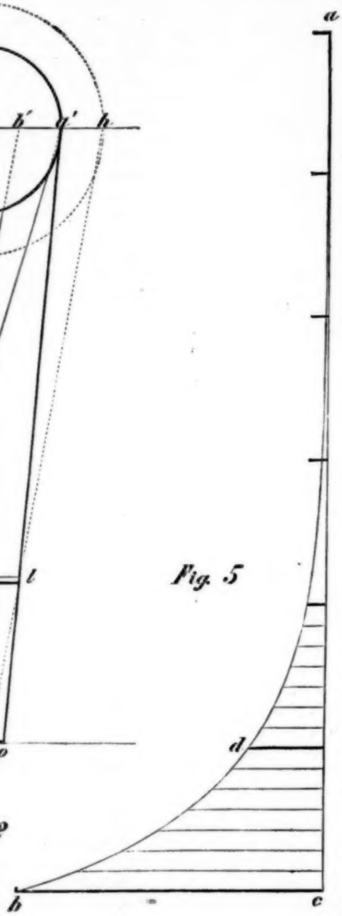
THE increase of the volume of atmospheric air, under constant pressure, being directly proportional to the increment of temperature, while the coefficient of expansion is 0·00203 for 1° of Fahrenheit, it will be seen that a temperature of 3,272,000° Fah. communicated to the terrestrial atmosphere would reduce its density to $\frac{1}{6643}$ of the existing density. Accordingly, if we assume that the height of our atmosphere is only 42 miles, the elevation of temperature mentioned would cause an expansion increasing its height to $6643 \times 42 = 279{,}006$ miles. This calculation, it should be observed, takes no cognizance of the diminution of the earth's attraction at great altitudes, which, if taken into account, would considerably increase the estimated height. Let us now suppose the atmosphere of the sun to be replaced by a medium similar to the terrestrial atmosphere raised to the temperature of 3,272,000°, and containing the same quantity of matter as the terrestrial atmosphere for corresponding area. Evidently the attraction of the sun's mass would under these conditions augment the density and weight of the supposed atmosphere nearly in the ratio of 27·9 : 1; hence its height would be reduced to $\frac{279{,}006}{27{\cdot}9} = 10{,}000$ miles. But if the atmosphere thus increased in density by the sun's superior attraction consisted of a compound gas principally hydrogen, say 1·4 times heavier than pure hydrogen, the height would be $10 \times 10{,}000 = 100{,}000$ miles. The pressure exerted by this supposed atmosphere at the surface of the photosphere would obviously be $14{\cdot}7 \times 27{\cdot}9 = 410$ pounds per square inch, nearly. Unless, therefore, the depth greatly exceeds 100,000 miles, and unless it can be shown that the mean temperature is less than 3,272,000° Fah., the important conclusion must be accepted that the solar atmosphere contains so small a quantity of matter that notwithstanding the great depth it will offer only an insignificant resistance to the passage of the solar rays. Now, the assumed mean temperature, 3,272,000°, so far from being too high, will be found to be considerably underrated. It will be recollected that the temperature at the surface of the photosphere, determined by the ascertained intensity of solar radiation at the boundary of the earth's atmosphere, somewhat exceeds 4,035,000°. Consequently, as the diminution of intensity caused by the dispersion of the rays, will be inversely as the convex areas of the photosphere and the sphere formed by the boundary of the solar envelope, viz., 1·52 : 1, the temperature at the said boundary will be

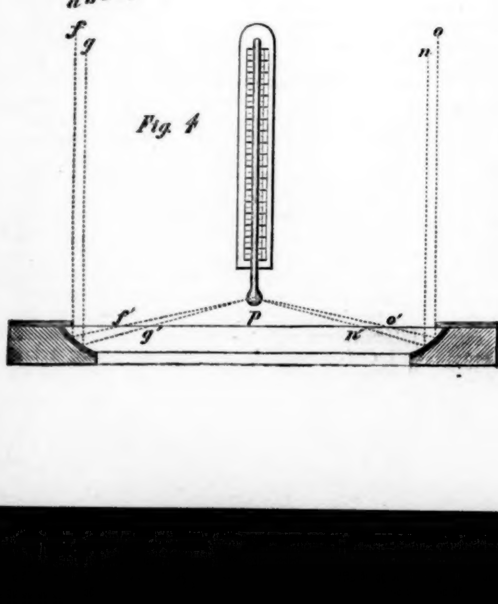
$$\frac{4{,}035{,}000^\circ}{1{\cdot}52} = 2{,}654{,}600^\circ$$

The true mean, therefore, will be 3,344,800°, instead of 3,272,000° Fah., a difference which leads irresistibly to the inference that, either the solar atmosphere is more than 100,000 miles in depth, or it contains less matter than the terrestrial atmosphere, for corresponding area. It will be demonstrated hereafter that the retardation of the rays projected from the border of the photosphere consequent on the increased depth of the solar atmosphere (supposed to be the main cause of the observed diminution of energy near the sun's limb), cannot appreciably diminish the intensity of the radiant heat. The ratio of diminution of the density of the gases composing the solar atmosphere at succeeding altitudes, is represented by Fig. 5, in which the length of the ordinates of the curve *a d b* shows the degree of tenuity at definite points above the photosphere. This curve has been constructed agreeably to the theory that the densities at different altitudes, or what amounts to the same, the weight of the masses incumbent at succeeding points, decreases in geometrical progression as the height above the base increases in arithmetrical progression. The vertical line *a c* has been divided into 42 equal parts, in order to facilitate comparisons with the terrestrial atmosphere, the relative density of which, at corresponding heights, is obviously as correctly represented by this diagram as that of the solar atmosphere. It is true that, owing to the greater height of the latter compared with the attractive force of the sun's mass, the upper strata of the terrestrial atmosphere will be relatively more powerfully attracted than the upper strata of the vastly deeper solar atmosphere. The ordinates of the curve *a d b* will therefore not represent the density quite correctly in both cases. The discrepancy, however, resulting from the relatively inferior attraction of the sun's mass at the boundary of its atmosphere, will be very nearly neutralised by the increased density towards that boundary, consequent on the great reduction of temperature—fully 1,380,000° Fah.—caused by the dispersion of the solar rays before entering space. It may be well to add that, in representing the relative height and pressure of the terrestrial atmosphere, *a c* in our diagram indicates forty-two miles, while *b c* indicates a pressure of 14·7 pounds per square inch; and that in representing the solar atmosphere, *a c* indicates 100,000 miles and *b c* 410 pounds per square inch. Bearing in mind the high temperature and small specific gravity, the extreme tenuity in the higher regions of the solar atmosphere will be comprehended by mere inspection of our diagram. Already midway towards the assumed boundary, the density of the solar atmosphere is so far reduced that it contains only $\frac{1}{132000}$ of the quantity of matter contained in an equal volume of atmosphere at the surface of the earth.

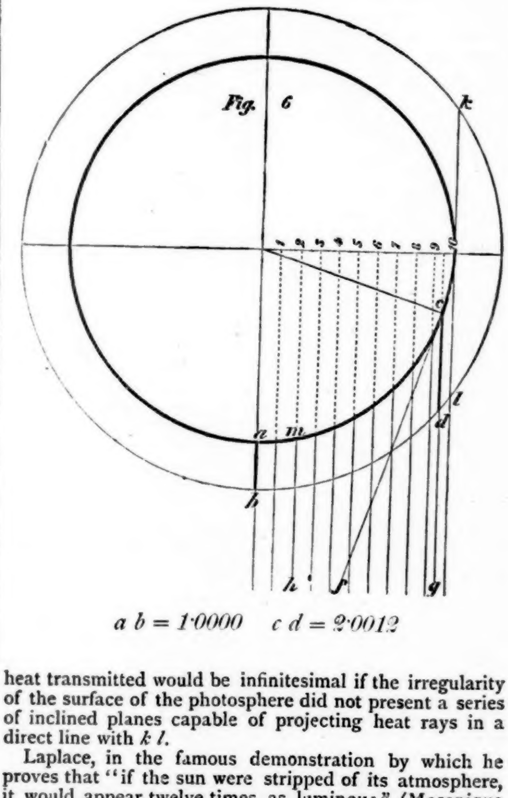
Let us now consider the diminution of intensity occasioned by the increased depth through which the heat rays pass which are projected from the receding surface of the photosphere. Fig. 6 represents the sun and its atmosphere extending $\frac{1}{4}$ of the semi-diameter of the photosphere, *m h*, *c g*, &c., &c., being the heat rays projected towards the earth. The depth of the solar atmosphere at a distance of $\frac{18}{20}$ of the radius from the centre of the luminary, will be seen to be only 2·0012 greater than the vertical depth. Now, careful actinometer observations enables us to demonstrate that when the zenith distance is under 60°, the radiant energy of the sun's rays in passing through the terrestrial atmosphere is very nearly in the inverse ratio of the cube root of the depth penetrated (see the previously published table). The increase of depth resulting from atmospheric refraction, it may be well to observe, is too small at moderate zenith distances to call for correction; nor does the atmospheric density vary sufficiently during bright sunshine to affect the radiant intensity appreciably. The table adverted to shows that an increase of the sun's zenith distance of 5′ in 60° occasions a diminution of temperature hardly amounting to 0·044° Fah. Adopting the same rate of retardation for the solar atmosphere as that observed in the terrestrial atmosphere, it will be found that the loss of radiant energy of the solar rays at $\frac{1}{20}$ of the radius from the border of the photosphere will be only 1·26 greater than at its centre. According to the researches of Secchi and others, the loss is fully three times greater than that established by the rate of diminution which we have adopted. This circumstance, in connection with the extreme tenuity of the solar atmosphere, rendering any considerable loss improbable, points to the fact that some other agency than increased depth is the true cause of the diminution of the temperature under consideration. Accordingly, the writer some time ago instituted a series of experiments with incandescent cast-iron spheres, for the purpose of ascertaining practically if the reduction of temperature could be accounted for solely on the ground that the obliquity of the rays diminishes their energy. Previous experiments had demonstrated that the accepted doctrine is quite incorrect, which teaches that heat rays emanating from the surface of incandescent radiators are projected with equal energy in all directions. It was found during those







experiments that the ratio of diminution of radiant heat transmitted to a stationary thermometer by an incandescent circular disc of cast-iron, turning on appropriate journals, is directly proportional to the sines of the angles formed by the face of the disc and lines drawn to the centre of the bulb of the stationary thermometer. It was clearly shown that those heat rays only which are projected at right angles to the face of the incandescent radiator, transmit maximum energy. The important bearing of this fact with reference to temperature transmitted by the heat rays of the photosphere from points near the border, is self-evident. The small angle formed by the ray *c g*, Fig. 6, and the tangent *c f* of the surface of the photosphere at *c*, explains satisfactorily why the radiant heat at a distance of $\frac{1}{20}$ of the radius from the sun's border, is considerably less than at the centre. It will be perceived that the angle *f c d* diminishes very rapidly as the border of the photosphere is approached, and that when the extreme point is reached, the radiant



$a\,b = 1{\cdot}0000 \quad c\,d = 2{\cdot}0012$

heat transmitted would be infinitesimal if the irregularity of the surface of the photosphere did not present a series of inclined planes capable of projecting heat rays in a direct line with *k l*.

Laplace, in the famous demonstration by which he proves that "if the sun were stripped of its atmosphere, it would appear twelve times as luminous" (Mecanique céleste, tom iv., pp. 284—288), commits the grave mistake of assuming that all rays emanating from a radiant surface possess equal energy. This assumption leads him further to the erroneous conclusion that the rays projected from the retreating surface of the sun near the limb, act as rays from a lens, being crowded together in consequence of the obliquity of the radiant surface, thereby, he supposes, acquiring increased intensity—hence the monstrous assertion of the great mathematician that, but for the interference of the solar atmosphere, the luminosity would be eleven times more intense.

The important question whether the solar atmosphere possesses any appreciable radiant power, and whether the

high temperature of the attenuated matter of which it is composed exercises any marked influence on the sun's radiant energy, may unquestionably be answered practically. An investigation, based on the expedient of concentrating the heat rays of the chromosphere by means of a parabolic reflector, has been conducted by the writer for some time. The method adopted is such that only the heat rays, if such there be, from the chromosphere and exterior solar envelope, are reflected; while the rays from the photosphere are effectually shut out. Fig. 1 shows the general arrangement; f' a' represents the photosphere, and g' h the boundary of the surrounding atmosphere; k l is a circular screen exactly 10 inches in diameter, placed 53·76 inches above the base line a o. This distance obviously varies considerably with the seasons. Assuming that the investigation takes place when the sun subtends an angle of 32′ 1″, the screen k l, if placed at the distance mentioned, will throw a shadow, f o, exactly 9·5 inches diameter; hence objects in the plane a o placed within f o, will be effectually shut out from the rays projected by the photosphere, while they will be fully exposed to the rays, if any, emanating from the chromosphere and outer strata of the solar envelope. It should be observed that, owing to diffraction in connection with the extreme feebleness of the sun's rays projected from the border, the shadow thrown by the screen k l extends considerably beyond the circular area defined by f o. Fig. 3 exhibits a *full size* segment of this shadow as it appears round f o, the section coloured black in Fig. 2 being a photometric representation of the strength of the said shadow from f to a. Special attention is called to this photometric representation, as it shows that objects placed within the circular area defined by f o are absolutely screened from the rays of the photosphere. It is evident that a parabolic reflector of proper size placed immediately below f o, will concentrate the radiant heat, if any, transmitted by the rays f' f and g' g and the intermediate rays. Fig. 4 represents a section of the parabolic reflector which has been employed during the investigation. It consists of a solid wrought-iron ring lined with silver on the inside, turned to exact form and highly polished. An annular plate 9·5 inches internal diameter, is secured to the top of the wrought iron ring to prevent effectually any rays from the photosphere reaching the reflector. The prolongation of the rays f' f - g' g and h n - a' o are shown by dotted lines f, g and n, o; also the reflected rays directed towards the bulb of the focal thermometer, marked respectively f', o' and g', n'. The investigation not being yet concluded, the following brief account is deemed sufficient at present. Turning the reflector towards the sun, without applying the screen k l, a narrow zone of dazzling white light is produced on the black bulb of the focal thermometer, the mercurial column commencing to rise the moment the rays strike the reflecting surface. With a perfectly clear sky, the column during an experiment on August 29, 1871, reached 320° Fah. in thirty-five seconds. The screen k l being applied, after cooling the thermometer, a zone of feeble grey light appeared on the black bulb nearly as wide as the one produced by the rays from the photosphere, but situated somewhat lower. The column of the focal thermometer, however, remained stationary, excepting the oscillation which always takes place when a thermometer is subjected to the influence of the currents of air unavoidable in a place exposed to a powerful sun. It is proper to remark that owing to the stated oscillation, it cannot be positively asserted that there was no heating whatever produced by the reflection and concentration of the rays which formed the zone of grey light adverted to. But the recorded oscillations prove absolutely that the heating did not *exceed* 0·5° Fah. Assuming that such a temperature was actually produced by the reflected concentrated heat emanating from the solar envelope, the following calculation will show that the energy thereby established is too insignificant to exercise any appreciable influence on the sun's radiant power. Theoretically, the temperature transmitted to the bulb of the focal thermometer by the rays f and o, Fig. 4, is inversely as the foreshortened illuminated area of the reflector to the zone of light produced on the bulb. Obviously these areas bear nearly the same relation to each other as the squares of f' or o' to the square of the radius of the bulb p. The length of f' being 4·77in., while the radius of the bulb is 0·125in.; calculation shows that the temperature transmitted by the ray f would be increased 1,456 times if the reflector did not absorb any heat. Allowing that 0·72 of the heat is reflected, the augmentation of intensity by concentration will amount to 0·72 × 1,456 = 1,048 times the temperature transmitted by the rays f and o. The records of the oscillations of the mercurial column during the experiments show, as stated, that the temperature resulting from concentration cannot exceed 0·5°, hence the temperature transmitted by the rays emanating from the heated matter of the solar envelope will only amount to $\frac{1}{2 \times 1084}$ = 0·00047° Fah. The observations having been made when the sun's zenith distance was 32° 15′, a correction for loss occasioned by retardation amounting to 0·26 will, however, be necessary. This correction being made, it will be found that the heat actually transmitted by the rays from the solar envelope during the experiment of August 29, did not exceed 0·00059° Fah., a fact which completely disposes of Secchi's remarkable assumption that the high temperature of the photosphere is owing to the "radiation received from all the transparent strata of the solar envelope" (see his letter to NATURE, published June 1, 1871). But we are not discussing the cause; the degree of temperature at the surface of the photosphere is the problem to be solved.

It was stated in the previous article that the radiant power of incandescent metals and metals coated with lamp-black and maintained at boiling heat, is directly proportional to the temperature of the radiator. A series of experiments with flames just concluded, proves positively that under similar conditions a given area of flame of uniform intensity transmits the same temperature as incandescent cast-iron. Secchi's assertion, therefore, that the photosphere, if composed of incandescent gases, "may have a very high temperature and yet radiate but very little," is wholly untenable. The diminution of intensity attending the passage of the heat rays from the photosphere through the surrounding atmosphere, is the only point which can materially affect the question of temperature. We have shown that on a given area, the quantity of matter contained in the solar atmosphere cannot greatly exceed that of the terrestrial atmosphere; hence the retardation cannot be great. True, the depth of the solar envelope is vast compared with that of the earth's atmosphere, but distance *per se* does not affect the propagation of radiant heat. Admitting, however, the retardation to be as the cube root of the depth—the ratio observed in the terrestrial atmosphere—it will be found that the loss of energy produced by retardation of the heat rays is not important. The solar atmosphere being $\frac{100{,}000}{42}$ = 2381 times deeper than the earth's atmosphere, the retardation caused by the former will be 13·3 times greater than that of the terrestrial atmosphere, which, as we know, diminishes the radiant intensity 17·64° on the ecliptic. Accordingly we are justified in asserting that 13·3 × 17·64° = 234·6° Fah. will be the greatest possible diminution of temperature caused by the retarding influence of the matter composing the solar envelope. The admission in the previous article, that the retardation under consideration might be 0·01, was based on the extreme assumption that the obstruction is directly

proportional to the depth of the sun's atmosphere. At first sight the loss of 234·6° appears to be a trifling reduction of energy; yet if we consider the mechanical equivalent which it represents, we cannot doubt its adequacy to supply the motive force expended in producing the observed movement of the attenuated matter within the solar atmosphere. Dividing the temperature of the photosphere, 4,035,000°, by 234·6, it will be found that the computed, apparently insignificant, retardation exceeds $\frac{1}{17,000}$ of the entire dynamic energy developed by the sun—an amount fully 15,500 times greater than the solar energy transmitted to all the planets of our system! Making due allowance for the extreme attenuation, and the small quantity of matter to be moved, the most exaggerated computation of the probable expenditure of mechanical energy called for in keeping up the currents of the solar atmosphere, fails to establish an amount at all equal to that capable of being generated by utilising 234° of the radiant heat emanating from the photosphere.

In view of the foregoing statements and the demonstrations contained in the previous article on solar heat, we cannot consistently refuse to accept the conclusion, that the temperature at the surface of the photosphere is very nearly 4,036,000° Fah. J. ERICSSON

NOTES

THE Regius Professor of Physic at Cambridge (Dr. Bond) has issued a schedule of lectures on subjects connected with the study of medicine which will be delivered during the Academical year 1871-2. The following are the arrangements for this Term: Prof. Liveing will lecture on practical chemistry on Tuesdays, Thursdays, and Saturdays, at 1 P.M., commencing October 10. The Linacre lecturer will deliver a course of medical clinical lectures on Fridays, at 10 A.M., commencing October 13. The Professor of Anatomy (Dr. Humphry) will lecture on practical anatomy on Mondays, Wednesdays, and Fridays, at 6 P.M., commencing October 16. Mr. C. Lestourgeon, M.A., will on October 19 commence a course of surgical clinical lectures, and will continue the same on each Thursday during Term, at 11 A.M. Anatomy and Physiology will be the subject of a course by the Professor of Anatomy, commencing October 21, at 1 P.M., and continued on Tuesdays, Thursdays, and Saturdays at the same hour. The Professor of Zoology and Comparative Anatomy (Mr. A. Newton) will lecture on those subjects on Mondays, Wednesdays, and Fridays, at 1 P.M., commencing October 23. Special departments in chemistry will be the subject of lectures by the professor of that faculty on Tuesdays, Thursdays, and Saturdays at noon, commencing October 26. Practical histology will form a separate course under the superintendence of Dr. Humphry, commencing October 28 at 11.30 A.M., and continued each succeeding Saturday until its completion.

THE Franklin Institute of Philadelphia announces the following synopsis of lectures for 1871-72. The regular course will comprise a series of forty lectures, divided as follows:—1. "On Physics and Mechanics," by John G. Moore, M.S. 2. "On General Physics and Acoustics," by Prof. Edwin J. Houston. 3. "On Guns, Gunpowder, and Projectiles," by Lieut. C. E. Dutton. 4. "On the Chemistry of the Earth and of the Vital Process in Animals and Plants," by Prof. Samuel B. Howell, M.D. 5. "On the History of Alchemy," by William H. Wahl, Ph.D. 6. "On the Metallurgy of Iron and Steel," by Thos. M. Brown, Ph.D. Besides the lectures enumerated, the Institute has arranged with a number of eminent lecturers for the delivery of a popular course of scientific subjects, and it is believed that the plan here indicated, of offering a series of lectures brilliantly and largely illustrated, will go far towards attracting the attention and interest of the public to these most important subjects.

THE Managers of the London Institution, Finsbury Circus, announce the following programme of lecture arrangements for the coming season. The courses of educational lectures will be as follows:—First course, commencing Monday, October 30: Eight lectures "On Elementary Physiology," by Prof. Huxley. Second Course, commencing Monday, January 15, 1872: Eight Lectures "On Elementary Chemistry," by Prof. Odling. Third Course, commencing Monday, March 11, 1872: Six lectures "On Elementary Music," by Prof. Ella, director of the Musical Union. Fourth Course, commencing Monday, April 29, 1872: Six Lectures "On Elementary Botany; with special reference to the Classification of Plants," by Prof. Bentley. A Course of Four Lectures, adapted to a juvenile auditory, "On the Philosophy of Magic," by Mr. J. C. Brough, principal librarian in the London Institution, will be commenced on Thursday, December 21. A Course of Two Lectures "On Science and Commerce; illustrated by the Raw Materials of our Manufactures," by Mr. P. L. Simmonds, will be commenced on Thursday, November 23. This course will be illustrated by a large collection of beautiful and interesting specimens of animal and vegetable products. The following lectures will probably be delivered at the Conversazioni of the coming season:—"The Teachings of the Spectroscope," by Dr. William Huggins; "The Homing, or Carrier Pigeon: its Natural History, Training, and Exploits," by Mr. W. B. Tegetmeier; "The Sun," by Mr. J. Norman Lockyer; "Two Years' Gleanings in Syria and Palestine," by Captain Richard F. Burton; "The Haunts of Old Londoners," by Mr. Thomas Archer; "On Colour," by Prof. Barff. The evening class for elementary chemical analysis will commence work, under the direction of Prof. H. E. Armstrong, on Tuesday, November 7.

IN his address at the recent opening of the new Mechanics' Institute at Bradford, Mr. W. E. Forster, M.P., remarked that when institutions of this kind were first established they were intended to give to mechanics scientific knowledge; but it was discovered that that was impossible, except in rare cases, because mechanics had no elementrary teaching on which could be grounded scientific knowledge, and consequently these institutes were obliged to be turned very much into elementary schools and night schools, rather than into the teaching of science and higher literature, which we had hoped to give to our mechanics. A conviction, however, is now gaining ground that an essential portion of this elementary teaching consists of instruction in the rudiments of science, which would be of material advantage to none more than to the working classes.

THE open Scholarship in Natural Science, established this year at St. Mary's Hospital, has been gained by Mr. E. J. Edwards. This Scholarship is worth 40*l.* a year for three years. The Exhibition of 20*l.*, awarded at the same time, has been gained by Mr. Giles. Both gentlemen are students at the University of London.

THE Ettles Scholarship at the University of Edinburgh, which is annually awarded to the most distinguished graduate, has been given to Dr. Urban Pritchard, a student of King's College, London. Dr. Pritchard also gained a gold medal for original researches on the structure of the organ of Corti, conducted by him in the physiological laboratory of King's College.

THE vacancy in the Botanical Department of the British Mseuum, caused by the promotion of Mr. Carruthers, has been filled by the appointment of Mr. James Britten, late assistant in the Royal Herbarium, Kew.

MR. ROBERT ROUTLEDGE, a scientific graduate in honours of the University of London, has been appointed conductor of the classes in Chemistry and Physical Science at the Manchester Mechanics' Institute. These classes are intended to encourage technical education among the working classes, and consist of

courses on applied mechanics, steam and the steam-engines, acoustics, light and heat, magnetism and electricity, inorganic chemistry, and practical chemistry, held in the evening, and fully illustrated by experiments, diagrams, and models. The fees for members of the institution are, with the exception of the class of Practical Chemistry, one shilling per session.

WE regret to hear from German advices, of the death of Prof. Schweigger-Seidel, of Leipzig, assistant Professor in Histology to Prof. Ludwig. Prof. Schweigger-Seidel was well known for his careful and accurate researches on several difficult points of histology, especially connected with nerve-endings in the salivary glands, the lymphatic system, and the cornea.

THE *Geological Magazine* records the death, at the age of thirty, of Dr. Georg Justin Carl Urbar. Schloenbach, Professor of Geology of the Polytechnic Institute of Prague. Previously to receiving this appointment, Dr. Schloenbach had resided in Vienna, where he was an active and energetic member of the k. k. Geol. Reichsanstalt. It was whilst engaged for this Institute, travelling in Servia, that his constitution broke down, under the tremendous fatigue which geologists in these parts have sometimes to undergo. Camping out in what is by no means a tropical latitude brought on rheumatism, and shortly afterwards congestion of the lungs ended his life, after a painful but short illness.

GOOD progress is reported from the Hartley Institute, Southampton, both the day and evening classes being in a very flourishing condition. During the past year as many as 420 students attended these classes. As Science forms a large proportion of the instruction given, there can be but little doubt that the value of the technical knowledge so disseminated will be very great.

THE next Actonian Prize or prizes offered by the Royal Institution, will be awarded in the year 1872 to an essay or essays illustrative of the wisdom and beneficence of the Almighty. The subject is "The Theory of the Evolution of Living Things." The prize fund is two hundred guineas, and it will be awarded as a single prize, or in sums of not less than one hundred guineas each, or withheld altogether, as the managers in their judgment shall think proper. Competitors for the prize are requested to send their essays to the Royal Institution, Albemarle Street, on or before June 30, 1872, addressed to the secretary, and the adjudication will be made by the managers in December 1872.

THE First Commissioner of Works and Public Buildings announces that he intends again to distribute this autumn, among the working classes and the poor inhabitants of London, the surplus bedding-out plants in Battersea, Hyde, the Regent's, and Victoria Parks, and in the Royal Gardens, Kew. If the clergy, school committees, and others interested, will make application to the superintendents of the parks nearest to their respective parishes, or to the director of the Royal Gardens, Kew, in the cases of persons residing in that neighbourhood, they will receive early intimation of the number of plants that can be allotted to each applicant, and of the time and manner of their distribution.

A ROYAL Commission has been appointed at Melbourne for Foreign Industries and Forests, the members being the Hon. S. H. Bindon, Chairman; the Hon. G. W. Cole, M.L.C.; the Hon. R. Hope, M.D., M.L.C.; Mr. R. Ramsay, M.P.; Mr. J. F. Levien, M.P.; Mr. W. Witt, M.P.; Mr. T. M. B. Phillips, M.P.; Mr. F. Von Mueller, C.M.G., F.R.S; Mr. Thos. Black, President of the Acclimatisation Society, M.D.; the Rev. J. I. Bleasdale, D.D.; Mr. Paul de Castella; Mr. C. Hodgkinson; Mr. R. Brough Smith, F.G.S.; Mr. John Hood. The objects of the Commission are to consider and report how far it may be practicable to introduce into that country branches of industry which are known to be common and profitable among the farming population of Continental Europe; to specify which of such industries are most suitable to the soil, climate, and circumstances; to report on the best means of promoting their introduction into Victoria; to report how far the labour of persons at the disposal of the State may be advantageously used for that purpose; to further consider and report on the best means of promoting the culture, extension, and preservation of State forests in Victoria; and to report on the introduction of such foreign trees as may be suitable to the climate and useful for industrial purposes.

THE Government of India have resolved to organise a statistical department for the purpose of ascertaining and conserving the internal resources of India. Dr. Hunter will be the first Director-General of this new department.

IT seems hardly credible that no public monument exists in this country to the discoverer of the circulation of the blood This defect is now likely to be remedied, and preliminary steps have been taken at Folkestone, Harvey's birthplace, to mark the tercentenary of his birth by the erection of a suitable public monument. At a meeting convened by influential requisition—the Mayor of Folkestone in the chair—Mr. George Eastes, M.B., with whom the movement originates, read an interesting sketch of his life, labour, and character. Dr. Bateman, Dr. Bowles, and other local gentlemen, moved resolutions appointing a numerous committee, nominating Dr. Bence Jones, F.R.S., treasurer, the Town Clerk of Folkestone and Mr. George Eastes, M.B., London, as honorary secretaries.

AT the last sitting of the French Academy, an important paper was read on the results of M. Pasteur's long and patient researches into the causes and the best mode of extirpating that terrible disease of the silkworm, the *pébrine*. His efforts appear to have been eminently successful in checking the epidemic, by the simple means of destroying the eggs from all moths which can by any possibility have become tainted. The yield of healthy eggs is now again increasing rapidly in the south of France; and in a few years the disease will probably be all but exterminated. It is hoped that when the National Assembly again meets, some public recognition will be made of M. Pasteur's eminent services.

THE *Observer* comments with great justice on the disproportion between the emoluments for divinity, and for legal, mathematical, and classical instruction at Oxford—"While the salaries of five legal professors, in the aggregate, reach 2,000*l*., those of the Latin and Greek professors reach 1,100*l*.; those of three professors of metaphysics, &c., reach 1,100*l*.; and those of three mathematical professors reach 1,400*l*.—showing an average of about 480*l*. for each professor; the six professors of divinity enjoy the munificent income of upwards of 1,000*l*. a year each, with houses into the bargain." It adds, "That Oxford should pay 6,300*l*. a year for doctrinal divinity, and only 500*l*. a year for Greek, is a quaint anomaly, to say the least." If, however, our contemporary had included statistics of the remuneration for science, it would have strengthened its case considerably.

THE *Journal of Botany* states that a great desideratum in botanical literature is shortly to be supplied. Considerable progress has been made in printing a second edition of Pritzel's "Thesaurus Literaturæ Botanicæ," a catalogue of all works ever published in all departments of botanical literature, now twenty years old.

WE have received from Mr. Marshall Hall a history of the cruise of the *Norna*, giving in a pleasant chatty form the main results of the expedition as they would interest the public at large. The more important zoological details will be found in another column.

WE are glad to observe that the conductors of the *Scottish*

Naturalist are able to announce that with the next number the size of the magazine will be increased to 40 pages. Several important and interesting contributions are announced for 1872; and we hope that this useful magazine will meet with the support and circulation that it deserves.

PROF. J. LAWRENCE SMITH, in the September number of the *American Journal of Science*, gives the following analysis of the meteorite stone which fell near Searsmont, Maine, on the 21st of May of this year:—

Nickeliferous Iron	14·63
Magnetic Pyrites	3·06
Olivine	43·04
Bronzite, a hornblende with a little albite or orthoclase and chrome iron	39·27

IT is stated that a crater of a new volcano has been formed on the mountain near Bivoria in the province of Girgenti in Sicily.

THE cyclone which visited St. Thomas and Antigua on the 21st of August, continued its course towards the Bahamas, and reached Turks Island on the 22nd. The storm occupied about eight hours in travelling from St. Kitts to St. Thomas, 150 miles, and so had a rate of progress of about 18¾ miles per hour, but from St. Thomas to Turks Island the velocity decreased to about 12¼ miles per hour, taking about 31 hours to travel 38c miles.

A SLIGHT shock of earthquake was felt at Kingston, Jamaica, at 4 P.M. on the 3rd of September.

THE star showers of the 10th and 11th of August last were attentively watched in America as in Europe. At Sherburne, New York, according to the American *Journal of Science*, a party of six persons watched between 11.40 and 12, and saw 48 meteors. In the next hour 143 were seen, and in the first eighteen minutes of the next hour 32. The latitude of the radiant point was 1⅓° less than that of the nebula in Perseus.

Les Mondes gives the particulars of a remarkable meteorite observed at Marseilles by M. Coggia, on the 1st of August. It made its appearance at 10h. 43m., Marseilles mean time, at a point situated near the centre of the triangle formed by ζ Serpentis and θ and η Ophiuchi. The course was remarkably slow, in an easterly direction; at 10h. 45m. 30s. it passed between μ_1 and μ_2 Sagittarii, and at 10h. 46m. 35s. it almost occulted Saturn. The course became then still slower; at 10h. 49m. 50s. it passed a little below o Sagittarii, and at 10h. 50m. 40s. south of the star f of the same constellation. At 10h. 52m. 30s. it passed between ι and θ Capricorni, where it remained for a moment stationary, then changing its course, it took a northerly direction, leaving at 10h. 57m. 50s. the star ν Aquarii 1° 30′ to the west, and again stopping, at 10h. 59m. 30s., a little south-west of β Aquarii. Regaining its original easterly direction, it then passed β Aquarii, stopping again near ζ Aquarii, and then fell rapidly in a perpendicular direction near δ Capricorni, and leaving to the east the almost full moon. It finally disappeared a little north of θ Pisc. austral. at 11h. 3m. 28s. The diameter, which was at first about 15′, diminished rapidly, was a little over 4′ when it approached Saturn, and finally had scarcely more than the apparent size of Venus. During its perpendicular fall to the horizon, it gave out vivid scintillations.

THE *Times of India* gives the following story:—Advices from Ihangara state that at a place about forty miles distant on the hills, a thunderbolt fell on the 22nd of August after a heavy downpour of rain. The ground was literally cut up in consequence, and the whole of the huts standing there as well as their inmates were swallowed up in the chasm. Such a catastrophe has never been known in Sind. Some fifty or sixty persons perished.

ON the 11th of July a strong shock of earthquake was felt at Valparaiso in Chile, preceded by a loud rumbling noise. On the 20th, at 11 P.M., a very severe shock was felt at Santiago de Chile.

THE following account of a hairy family appears in the *Indian Daily News*:—"The hairy family of Mandalay consists of a woman of about forty-five years of age, a man of twenty, and a girl of eleven, with hair over every part of their faces, forehead, nose, and chin, varying in length from three inches to a foot, and exactly the colour and texture of that on a skye-terrier. The hair of their heads, on the contrary, is just the same as on any ordinary Burman; they appear to be quite as intelligent as the ordinary Burmans. The father of the woman was the first of the hairy progeny. He married an ordinary Burman woman, and the issue of the union was the present hairy head of the family. She married an ordinary Burman, and has issue, a son about twenty-three years of age, not hairy, and the boy and girl alluded to. The Burmese explanation of the phenomenon is, to say the least, curious, and might possibly possess a special interest for Mr. Darwin. These hairy people would be worth a fortune to the enterprising Barnum if he could get hold of them, but the King will not allow them to go out of his dominions."

SCIENTIFIC INTELLIGENCE FROM AMERICA*

THE fourth Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology has made its appearance, and presents a gratifying picture of the progress of this great establishment. The most important additions during the year have been a collection of stone implements from Cape Cod presented by Mr. Samuel H. Russell, a series of duplicates from the Christie collection of London, and specimens obtained from explorations in Tennessee by Mr. Dunning, and in Central America by Dr. Berendt. These are supplemented by numerous single donations of greater or less value. In the course of some critical observations upon the specimens received by the Museum, attention is called to the great value of a collection of crania and human bones obtained from certain mounds in Kentucky by Mr. S. S. Lyon, in the course of explorations made under the combined auspices of the Smithsonian Institution and of the Peabody Museum. The peculiarities of the crania of the American Indians have already been referred to by various writers, but some curious facts are detailed in the report in regard to other portions of the skeleton. Thus the ulna and radius, as compared with the humerus, were found to be much larger in the mound Indians, while the length of the tibia, as compared with the femur, is longer in the whites. In quite an unusual number of Indian skeletons the two fossæ at the lower end of the humerus were found to communicate, producing a perforation. This feature, rarely met with in the white races, occurs quite frequently in the mound remains, while in the black race it appears to be still more frequent. An additional peculiarity of the mound bones consists in the flattening of the tibia, which, until the date of the present publication, has not been recorded as occurring in America, although remains from the dolmens of France, the quaternary drift of Clichy, and the burial caves of Cro-Magnon and Gibraltar, exhibit this in a very marked degree. As regards the pelvis, the breadth in the Indian races is found to be less than in the whites, while the three diameters of the brim of the true pelvis are greatest in the Indians. The transverse diameter and the size of the outlet of the pelvis are much the largest in the Indian, while the sacrum is less curved, supplying conditions which in the process of parturition are more favourable to the Indian women.—We have already referred at various times to enterprises on the part of the Peruvian Government in exploring the less-known portions of that country, and we find in late South American journals details of a movement looking toward the examination of the regions of the Ucayale and Urubamba. The object of the expedition is to find a port which will open up to the Department of Cuzco a communication with the main branch of the Amazon, and thence to the Atlantic. The work is to be under the direction of Mr. Tucker, favourably known in similar enter-

* Communicated by the Scientific Editor of *Harper's Weekly*.

prises before. The present plan is for Don Raymundo Estrella and another commissioner to start from the port of Illapani in two large canoes, and make their way by the Urubamba to Iquitos, which is the Peruvian naval station on the Amazon. This is for the purpose of obtaining such a knowledge of the rivers as may fit them to serve as pilots to the steamer which is to ascend the Ucayale and explore the Urubamba. They are to make their way back about thirty leagues from Cuzco.—The daily papers of August 29 contain the latest reports from Captain Hall and his steamer *Polaris*, in the form of a telegraphic despatch from the United States ship *Congress*, dated at St. John's, Newfoundland, August 28. It will be remembered that this vessel was detailed by the Secretary of the Navy to carry supplies of provisions and coal to be stored in Greenland for the use of the Arctic expedition. She left St. John's on her outward trip on the 3rd of August, reaching Disco on the 10th, passing hundreds of immense icebergs on the way. The *Polaris* was found at Disco, having reached that place only six days in advance, although she started long before the *Congress*. Captain Hall and his party were in good spirits, and sanguine of success. The *Congress* reports that Captain Hall left Disco on the 17th of August for the north, where communication with him will, of course, be uncertain for some time to come, unless the object of the expedition in reaching the north pole can be accomplished in time to return during the present year. It is understood that instead of going by way of Jones Sound, as was the original intention, Captain Hall will proceed along the eastern side of Smith Sound. By all accounts the water is much more open than for many years past, there being comparatively little drift-ice to bar progress. To the surprise of the officers of the *Congress*, the summer temperature of Greenland was found to be quite elevated, and there was a luxuriant vegetation to be seen around the settlement of Disco.—The Panama papers speak of the great success which several whaling ships are now meeting with in the Bay of Panama, quite a number of whales having been killed there every day for some time past. It is stated that at the time the steamship *Chile* passed Payta, a school of small whales had been there in such abundance that the boats were afraid to leave the harbour.—We have already referred to the hydrographical and other explorations in Alaska by Mr. William H. Dall, under the patronage of the Coast Survey; and we now learn that he left San Francisco for the north at the end of August, bound direct to Iliuliuk Harbour, Oonalaska, there to go into winter-quarters. It was his intention, according to his instructions, to make use of every favourable opportunity to survey the vicinity of that port, and in March to proceed westward, sounding and surveying as far as Kamtchatka, and then turning north and eastward to Cape Romanzoff, to return to Oonalaska, and thence proceed homeward. The vessel obtained for the expedition, although small, is conveniently adapted for its purpose, and can carry provisions for six months; and it is expected that fresh supplies will be forwarded from San Francisco in March next. The party, besides Mr. Dall, consists of Prof. Harrington, the astronomer, Captain W. G. Hall, sailing-master, with two mates and five men.

ON THE STUDY OF SCIENCE IN SCHOOLS *

II.

WE now come to the second heading of our discourse, viz., the objects and aims of the experimental sciences, and the reason why we study them. Now the main object of science is the discovery of new truths, and the destruction of old errors. The human mind, much as it loves truth, has in the course of ages given birth to an infinite number of fallacies, specially in regard to the operations of Nature. Fallacies handed down by tradition; fallacies elaborated in the mind of dreamers, and theorists, and believers in magic; fallacies founded upon inaccurate observation, false experiment, perverted reasoning; these have ever been the barriers which have most retarded the progress of true science; and the earlier natural philosophers had to contend against a mass of such pre-existent opinion and superstition. We can scarcely realise in the present day the amount of superstition which existed among all classes even two hundred years ago, and at an earlier period it was far more prevalent. That same Athanasius Kircher, who was before mentioned as the author of a book on light, and who also wrote on magnetism, gives a detailed account of an encounter with a dragon in one of the passes of the Alps, and illustrates his assertion by an exceedingly bold and imaginative woodcut. Metals were believed to be generated in the earth by the action of the sun. Gold had a large proportion of condensed sunbeams. A mine when exhausted was closed, and re-opened after some years in the hope that the metal would have been produced in the meanwhile. Many—among them Cardanus—believed that metals and minerals possessed a kind of life, and that certain changes in them, such as conversion into calx, were the result of their death. The air was peopled with invisible demons, who wrought all kinds of mischief, raised storms and whirlwinds, and warred against the works of man. Witches and wizards were in league with them, and could influence them, and were hence treated with extreme severity. In 1487 there was an unusually devastating storm in Switzerland, and two old women, who were believed to be witches, were arrested on the charge of having caused it. They of course denied the charge, but during the torment of the rack they confesssed they had raised the tempest. They were forthwith executed—"Convicta et combusta." These cases were by no means rare. Witches were believed to exist by the hundred and thousand, and to produce all kinds of supernatural effects. Pope Innocent VIII. issued a manifesto against them in 1488, and appointed inquisitors in all countries, armed with powers of arresting and punishing suspected sorcerors. In Geneva alone, no less than 500 persons were burned in 1515 and 1516. So late as the year 1716, two persons were executed in England for the practice of witchcraft. We can understand all this better if we bear in mind how much superstition still exists in the world. Not to mention those things which appear under pseudo-scientific names, we find in many out-of-the-way villages, specially in Ireland, a very firm belief among the uneducated in the power of charms, and the existence of witches. In a village not far removed from the outer world, a witch has been pointed out to me, and the laming of a horse and other disasters seriously attributed to her charge. Gaule, in his "Magastromancer," gives a list of *fifty-two* forms of divination, and he has omitted at least six which are found in the works of other writers. Among other forms we have divining by ashes, by smoke, by the lees of wine, by cheese, by figs, by knives and saws; you will remember also some of the forms of divination practised by the Romans. But perhaps the delusion which has most militated against the growth and progress of true natural science has been alchemy—a false science which flourished for more than 800 years, and which was firmly believed in by thousands. The alchemists devoted their lives mainly to the search for two palpable impossibilities; the Elixir Vitæ, which was believed to possess the power of conferring perpetual youth, and the Philosopher's Stone, which was believed to transmute everything that it touched into gold. The search for this substance, and the endeavours to make it by artificial means, occupied the attention of many notorious and eminent men. Albertus Magnus, who became Bishop of Ratisbon in 1259, and S. Thomas Aquinas, were particularly addicted to alchemy and magic. We hear most of their magical powers, although their writings on alchemy still remain. Between them they made a brazen statue and endowed it with the faculty of speech; but it was so garrulous that one day Thomas Aquinas, who was in vain trying to work out a mathematical problem, seized a hammer and destroyed it—at least, so say contemporary writers. Albertus Magnus once changed a severe winter into a most splendid summer within the space of his garden. Detailed accounts exist of the transmutation of lead and tin into gold. Raymond Lully states in one of his works that he converted 50,000 lbs. weight of quicksilver, lead, and pewter into gold. Pope John XXII. was a great alchemist, and had a laboratory at Avignon. He wrote a work on the transmutation of metals, and at his death left a sum of eighteen millions of florins, the existence of which according to contemporary alchemists, proved the possibility of transmutation. And thus one might continue to give a long list of known men who devoted themselves to these useless pursuits; and the unknown men could be counted by thousands. Here, then, we have some of the fallacies which it has been the object of science to disprove, and which, so long as they existed in full vigour, effectually prevented the progress of science. The disproval of these could only result in the discovery of new truths. There is an intense satisfaction in the discovery of absolute truth; truth which stands every opposition, which has been weighed in many balances and not found wanting; which has been submitted to every process of reasoning and of experiment, and has come out uninjured. Taking this discovery of new

* Conclusion of a Lecture delivered at Marlborough College as an introduction to the commencment of Science teaching, by G. F. Rodwell.

truths as the first and greatest aim of science, we may, perhaps, take next some of Francis Bacon's more practical ideas about the objects and aims of science; to increase man's sovereignty over Nature, to compel Nature to be subservient to his will, and to minister to his wants; to restore his lost sovereignty over Creation. And, indeed, when the new truths are discovered, they are soon applied to practical purposes, and to furthering the material good of mankind; but to study science with this object alone is usually pernicious, and always to be avoided.

Some of you will ask me the more direct use of science. I fear I cannot tell you much about this; I would rather refer you to some of the enthusiastic—I hope not exaggerated—articles which have appeared from time to time during the last few years in various journals and magazines. It is directly useful for the purpose of science scholarships at the Universities, which are much on the increase; also it forms a part of the examinations at Woolwich, and for the Civil Service. Scientific appointments are year by year becoming more numerous in this country and in India. Indirectly, science is useful to every one. I say I cannot tell you much about its direct and practical uses, because I believe that the main use of it is to cultivate a certain set of mental faculties, to induce a certain mode of thought. The modes, and tones, and phases of mental action are as diverse as the modes of bodily action, and just as we exercise one set of muscles by rowing, another by riding, and a third by walking, so do we exercise a certain set of faculties when we study classics, another set when we study mathematics, and a third when we study sciences. The cultivation of this habit of thought engenders among other things a habit of observation and a spirit of inquiry. Questions suggest themselves daily, for an answer to which we must apply to science. Why do winds blow and storms rage? What are day and night, summer and winter, sunshine and frost? Of certain common things we rarely think, or if we do we assign the simplest meaning to them. For how many centuries did not mankind believe the world to be flat, the sun to be a globe of fire quenched nightly in the western sea, the sky to be solid, and the stars set into it like gems! Savages still believe that the firmament is a solid dome, and the sun and moon living creatures who walk across it.

The third of our four divisions concerns the methods we shall follow in our study of the sciences discussed above. Firstly:—lectures. It is essential that you should see the various changes wrought upon or within matter; not alone hear about them or read of them. You must not only observe, but you must think of the experimental results; understand them; understand the means by which they are brought about. It will be well for you to take notes, roughly at first, to be copied out afterward, and extended from memory. It is a mistake to take very full notes during a lecture. They may become an almost verbatim report of the lecture; the spirit of the matter is lost because the mind is fixed upon a detail. Experiments also are often lost; and at the end a mass of writing remains, but no knowledge of the work done. It is preferable to write down headings of subjects; the pith and marrow of the subject matter only;—in a word, to make merely an outline of the picture, and to fill in the details afterwards from memory. Sketches of apparatus are always desired among the notes, also any general remarks, and queries. At the end of each lecture you will be questioned, and at the commencement of each lecture the matter of the preceding lecture will be recapitulated; at this time also your own queries will be answered. It is important that you should not allow any subject to be partially understood, or misunderstood. Make a note of any difficulty, and let it be cleared up at the commencement of the next lecture, or at some intermediate time. The misunderstanding of one important fact may render the right understanding of succeeding matter nearly impossible. Then, later in the half, I should like you to read in text-books about the subject of your lectures, and thus to supplement the lecture-work by book-work. The advantage of this will be very apparent when you are examined.

What we desire is that science shall grow up side by side with your other subjects of study, and enter into your daily life. It is thus only that it can possess any real vitality. And if any subject of study possesses not vitality—intense, active, exuberant vitality—it languishes, becomes unhealthy, weak, and ultimately useless. It resembles a tree which loses first one branch, then another, and then dies entirely. And when upon the tree of knowledge a new branch is grafted, we desire to see it growing up side by side with the great branches already there. Our school knowledge—the knowledge which in its entirety fulfils the conditions of that comprehensive word *culture*—must be one and undivided; hence a new subject can only flourish when it is woven completely into our school life, when it ceases to be regarded as a something extraneous and beyond the pale. I hope none of you are like the doctors of Salamanca when they were confronted with Columbus, or like the cardinals who passed judgment upon Jordano Bruno and Galileo.

I must add one word in conclusion as to the attitude of mind most conducive to a right study of natural science. In the first place it is necessary to free the mind from previous ideas and conjectures, and to neglect the evidence of the senses unsupported by extraneous means; thus the earth seems to be flat, and the sun to be a glowing disc which moves around it, yet research has proved that our senses here deceive us. Again, how difficult it is to realise the fact that two sounds may produce silence, two lights darkness, until it is experimentally proved that such is the case. It is hard to believe that the force which manifests itself by attracting light bodies when amber is rubbed, is identical with lightning, yet such has been proved to be the fact. We must clear our minds from preconceived opinions before we can profit much by the teachings of science.

Do not be discouraged by the apparent difficulty of science at starting; all things newly presented to the mind require the exercise of some effort before they can be grasped. If the current of our thoughts is to be diverted into a new channel, it must needs require some time to change it from its old course. Comfort yourselves with the knowledge that at the outset you know more true natural science than did Aristotle and all the great philosophers of antiquity. The very science which you learn almost as soon as you know the alphabet, the fundamental ideas about the earth, the sun, the moon, the air, places you at starting ahead, in the matter of science, of the flower of Middle Age erudition: Professors of the Sorbonne, Doctors of Salamanca, Monsignori of the Sacred College. If, at first, the path of science seems to wind uphill all the way, remember that when the toil is over the view from the summit is very glorious. The sun rises upon a new land infinitely vast, infinitely fertile; full of streams by the side of which you may wander, and see all nature reflected in their pure depths.

Above all things, I would ask you to study science reverently. Many of our studies concern the works of man, here we are dealing with the works of God, governed directly by His laws. Surely then it behoves us to bow our heads as we enter the portal of Nature, to be possessed of infinite humility, to assume no prying spirit of curiosity, to have no intellectual pride. Some of you no doubt remember Rembrandt's picture of the "Anatomic Lesson," and the calm, reverent, inquiring look of the students who surround the dead man; a sort of awe in the presence of the wonderful mechanism of the microcosm Man, as we must have awe in the presence of the macrocosm Nature. A something almost akin to the deisedaimonia of the Ancients; a reverential fear of that which is obscure, and but partly manifest. I know not whether the smaller and more obscure works of God do not convey this even more than those which are immeasurably greater. S. Augustine says, "Deus est magnus in magnis, maximus autem in minimis." We are scarcely more awed by the myriad stars and suns and systems around us than by the myriad atoms of which the smallest mass of matter consists, and which possess functions, attributes, actions, as definite in character, as varied in form, and as absolutely governed by immutable laws, as the members of systems comprising a million worlds, ten million miles away.

ZOOLOGICAL RESULTS OF THE 1870 DREDGING EXPEDITION OF THE YACHT "NORNA" OFF THE COAST OF SPAIN AND PORTUGAL *

AT the last meeting of this Association, held at Liverpool, I exhibited as one of the trophies of the *Norna* Expedition a new silicious sponge, to which I gave the name of *Pheronema Grayi*, or "the Portuguese Bird's-Nest Sponge;" and on this occasion the following is a brief synopsis of other leading novelties and more general results of the dredging cruise. A few preliminary remarks on the origin and object of the expedition may preface this synopsis.

* Communicated to the Biological Section of the British Association, Edinburgh, August 8, 1871.

To Mr. Marshall Hall, F.G.S., &c., who personally superintended the expedition, are due the thanks of the scientific world for having so generously devoted his yacht *Norna* to the purpose of scientific discovery. This gentleman had early in the year conceived the project of rendering science that service it is to be regretted so few owners of yachts are disposed to contribute; and to him I feel myself under the deepest obligations for the opportunities afforded me during this cruise of acquiring that practical information so keenly appreciated by every working naturalist. Nor must I forget here to associate with his the name of Mr. Henry Lee, F.L.S., the worthy president of the Croydon Microscopical Society, as one of the chief instigators of the scheme, and the person to whom I am especially indebted for my introduction to Mr. Marshall Hall, as one likely to make the most of the opportunities that would be afforded.

Having accepted the last-named gentleman's kind invitation to accompany him as naturalist in a small way to the expedition, it was decided I should memorialise the Council of the Royal Society for a grant to defray the heavier expenses of dredging and collecting apparatus. My application was most favourably received, thanks to the numerous kind scientific friends who supported it, and a sum of 50*l.* was immediately placed at our disposal for the purpose required. My indebtedness to the Royal Society for this liberal assistance has already been acknowledged, though I cannot permit so fit an occasion as the present to pass without once more endorsing it.

By the middle of May everything was prepared, the Trustees of the British Museum, on the especial recommendation of Professor Owen and Mr. Waterhouse, extending me an extra three weeks' leave of absence. The companionship and services of Mr. Edward Fielding were also fortunately secured, whose earlier dredging experiences with Mr. M'Andrew in the Red Sea seemed calculated, as they afterwards proved, to be of the most valuable assistance. Our time being limited, the west coast of Spain and Portugal was decided upon as a locality likely to yield us the most satisfactory zoological results, and on the recommendation of Mr. Henry Woodward we resolved first to proceed to Vigo Bay, where, in company with his lamented brother, Dr. S. P. Woodward, and Mr. M'Andrew, he had in the year 1856 obtained such abundant and valuable material. From thence it was proposed we should work our way down to Lisbon, our particular ambition being to reach the deep-sea fishing ground off Setubal, some twenty miles further south, from whence Prof. du Bocage, the talented conservator of the Lisbon Museum, had obtained specimens of the "Glass Rope Sponge" (*Hyalonema*), and numerous other novel treasures. On starting, we touched and remained a couple of days at Guernsey, and at that spot a few hours spent in shore-collecting rewarded us with the earliest substantial fruits of the expedition; seven more days brought us to Vigo, the point which constituted the first basis of our practical dredging operations.

A detailed list of the numerous species collected throughout the cruise being in course of preparation for the more technical and exhaustive report to be presented to the Royal Society, I here propose, commencing at the lowest animal group, to briefly enumerate some of the more important forms taken, adding such remarks on the characters or connecting circumstances which render them more especially deserving of attention. Of all, the subkingdom of the Protozoa has perhaps furnished us with the most abundant and valuable material, the sponge class in particular contributing many novelties. Before leaving British waters even, the few hours spent in shore-collecting at Guernsey, already alluded to, resulted in the accession of three new species of the genera *Isodictya* and *Hymeniacidon*, which I have placed at the disposal of my kind friend Dr. Bowerbank to be described by him in his supplementary volume of the "British Spongiadæ," now closely approaching completion. The moderate depths within the Laminarian and Coralline zones, from the shore line down to fifty fathoms, at which we collected and dredged in Vigo Bay, and afterwards further south in the neighbourhood of Setubal and the Sado river, proved remarkably productive of species belonging to the same group, as also to that of the Calcarea or calcareous spiculed sponges including *Sycon* and *Grantia*, &c. The most interesting of any, however, were the species belonging to the Hexactinellidæ, or hexradiate spiculed sponges, of which the beautiful *Euplectella* and *Hyalonema* form familiar examples. Nine species belonging to this group were obtained at a depth varying from 400 to 800 fathoms off Cape Espichel and Cezimbra, including *Hyalonema*, *Dactylocalyx*, *Aphrocallistes Bocagii*, *Lanuginella pupa*, and four other species new to science, three out of which necessarily constitute the types of new genera, the residue again furnishing data enabling us better to appreciate the characters and distinctions of those previously made known to us. The form belonging to the same group, and described by myself as *Pheronema Grayi*, and exhibited at the last meeting of this Association, is the most conspicuous among all these on account of its size, and I would here add a few more words in reference to this particular type. Since last year I have been afforded the opportunity of examining and comparing my own with numerous specimens of Prof. Wyville Thomson's *Holtenia Carpenteri* taken in the North Sea and also in the Atlantic, and from an evolutionist's point of view, this examination has led me to regard my specimens as holding rather the rank of a well-marked local variety than of a distinct species as I at first premised. A comparison of the specimens, now placed side by side in the British Museum collection, will, I think, suffice to prove to all those interested in this subject how strongly marked as varieties these two forms are. Meanwhile, the generic name of *Pheronema* adopted by myself I still retain, as I consider both Prof. Wyville Thomson's form and my own to be local varieties of another species first described by Dr. Leidy of Philadelphia as *Pheronema annæ*, and a letter recently received from Dr. Leidy himself more fully convinces me of this, though he has not yet bestowed on it the minute microscopical investigation of its structure needed for the effectual clearing up of this, at present, doubtful point.

In my description of other sponges belonging to this same Hexactinellate group, read before the Royal Microscopical Society, and published in their "Transactions" for November 1870, I have, in creating a new genus and species, *Askonema setubalense*, erroneously associated Prof. Thomson's name with it as having once pronounced the form to be of vegetable and not animal organisation. The mistake arose from the misconception of a name singularly similar in euphony as pronounced to me by Prof. du Bocage, and I here avail myself of the opportunity of rendering Prof. Wyville Thomson that *amende honorable* I feel myself in duty bound to accord to him.

Passing next to the class of the Foraminifera, our gatherings have been remarkably rich both from the coralline and abyssal zones, the latter furnishing us with numerous arenaceous types (*Rhabdomina*, &c.), and the former being notably abundant in species and varieties of *Lagena* and *Cristellaria*. Many of these forms are new to science and await description, and I must not forget to acknowledge here my indebtedness to Mr. Henry Lee for the very great assistance he has rendered me in his skilful preparation of the various gatherings of these minute organisms. To Mr. Henry Hailes also my best thanks are due for similar services.

The Cœlenterate sub-kingdom has likewise furnished several new and rare forms, including among the latter category an example of *Hyalopathes pyramidalis*, M. Edw., one of the Antipathiidæ now represented for the first time in our national collection, if not in this country. In the Alcyonarian group, *Veritillum cynomorium*, first taken sparingly in Vigo Bay, and afterwards abundantly in the Laminarian zone near Setubal, excited our warmest admiration.

Nothing can exceed the beauty of the elegant opaline polypes of this zoophyte when fully expanded, and clustered like flowers on their orange-coloured stalk; a beauty, however, almost equalled by night when, on the slightest irritation, the whole colony glows from one extremity to the other with undulating waves of pale green phosphoric light. A large bucketful of these Alcyonaria was experimentally stirred up one dark evening, and the brilliant luminosity evolved produced a spectacle too brilliant for words to describe. The supporting stem appeared always to be the chief seat of these phosphorescent properties, and from thence the scintillations travelled onwards to the bodies of the polypes themselves. Some of the specimens of this magnificent zoophyte measured as much as ten inches from the proximal to the distal extremity of the supporting stalk, while the individual polypes, when fully exserted, protruded upwards of an inch-and-a-half from this inflated stalk, and measured as much as an inch in the diameter of their expanded tentacular discs.

Numerous polyzoa were also dredged up from the various depths, many of which remain yet to be identified; but the allied group of the Tunicata has perhaps furnished by far the most interesting material of the whole molluscoidan sub-kingdom; surface-skimmings one morning near the mouth of the Sado river having rewarded us with numerous specimens of an *Appendicularia*, which, from notes and sketches made at the time of their capture, I have since found to have presented phenomena seemingly not yet observed by any other naturalist. Hitherto these organisms have been presumed to constitute a distinct genus of

Tunicata *inter se*, or otherwise to be the larval conditions of higher forms. My own observations, however, recorded in the ast July number of the "Quarterly Journal of Microscopical Science," have led me to believe that they are the free swimming reproductive Zooids of higher Tunicates, bearing the same relation to them as many free swimming *Medusæ* do to some stationary hydroid colony. At the greater depth of 600 and 800 fathoms, various species of *Terebratulæ* were taken as representative of the class Brachiopoda.

Ascending yet higher to the subkingdom of the Mollusca, a large variety of interesting species rewarded our researches. Included among these were—*Fusus contrarius*, a common fossil of the Norfolk crag recently discovered in the living state in Vigo Bay by Mr. M'Andrew, and dredged by us in the same locality; also a species of *Cassis*, remarkable from its being more closely allied to *C. Saburon* and other species inhabiting the Japanese and Chinese seas than to any of its Mediterranean or Atlantic congeners. This circumstance of its affinity is the more remarkable when associated with the occurrence of a species of *Hyalonema* (*H. lusitanica*) off the samecoast, likewise scarcely distinguishable from the more familiar Japanese form *H. Sieboldi*.

The Annelida and Crustacea have also furnished a fair quota of new and interesting species, to be reverted to hereafter; and neither taking a step further onwards to the higher vertebrate sub-kingdom has good fortune entirely deserted us Availing ourselves, through the kind assistance of Prof. du Bocage, of the aid of the native fishermen and their appliances, we secured examples of several rare species of the deep-sea ground-sharks frequenting the Portuguese coast line; and among others a fine specimen of *Pseudotriakis microdon*, a species recently discovered and described by Prof. du Bocage and his gifted collaborateur, Felix de Brito Capello.

Generalising from the whole amount of material collected during our cruise off the Iberian coast, our plunder may be separated into two very distinct groups. One of these, including that collected from the shore line down to a depth of 100 fathoms, presenting an interblending of Mediterranean species with those prevalent on our own more temperate coasts. Among these former I may more especially mention the occurrence of *Dendrophyllia ramea*, a well-known Mediterranean branching coral in great luxuriance at the mouth of the river Sado, this being, I think, the first record of this coral being taken so far north, and also from the same locality *Calappa granulata*, *Maia verrucosa*, *Murex trunculus* and *brandaris*, *Cestum veneris*, *Veritillum cynomorium*, and numerous other species belonging to the various Invertebrate divisions usually regarded as confined to the same more southern area. The residue and far smaller assemblage of species embraces those derived from the abyssal depths of from 400 to 800 fathoms, and all these, including many forms new to science, are characterised by their boreal or cold area facies, and in this respect contribute further evidence in support of the deductions arrived at by Dr. Carpenter, from his own more extended researches into the fauna of these same great depths in connection with the important expeditions of the *Porcupine* and *Lightning*, and with which his name and those of his indefatigable colleagues, Prof. Wyville Thomson and Mr. Gwyn Jeffreys, are so worthily connected.

In conclusion, it is my sincere hope that the rich reward attending our own humble efforts may stimulate other yacht owners to follow the example of my esteemed friend, Mr. Marshall Hall, influencing them likewise to devote their craft for one or a portion of a season to the cause of science, and to the exploration of those new deep-sea fields of discovery, now waiting to yield up the richest treasures to each earnest worker. Such men will find themselves more than compensated for the sacrifice of time or other interests by the fascinating nature of the work they undertake, in addition to earning for themselves the lasting gratitude of the scientific world.

Our well-appointed and expensively-fitted-out Government expeditions should explore the remoter depths; but British pluck and private enterprise should esteem it their especial privilege to unfold to us the yet hidden mysteries of the ocean world nearer home; and if, again, all shall not succeed in discovering new phases of animal life, there is much and even more important work to be effected in ascertaining accurately the bathymetrical range and geographical limits and distribution of those forms already known to us.

W. SAVILLE KENT

PROF. BASTIAN ON THE GERM THEORY*

EPIDEMIC and acute diseases have many characters in common; they constitute a family the members of which are united by a certain bond of unity, though at the same time they are in other respects strikingly different from one another. The "general" character of the symptoms originally gave rise to the notion that these affections were in the main dependent upon changes in the nature and quality of the blood. This view is still the one most commonly entertained, and which seems most likely to be true. And seeing that particular sets of symptoms recur with as much definiteness as individual differences of constitution will permit, we have a right to believe that the changes in the blood—however induced and of whatsoever nature they may be—are definite and peculiar for each of these diseases. The successive changes in the blood which are the immediate causes of the phenomena of small-pox, must be quite different from those giving rise to the morbid state known as typhoid-fever. Variable as these several groups of symptoms are amongst themselves in individual cases, yet is there a general resemblance which suffices to maintain the distinctive nature of each affection. In this broad sense they are undoubtedly entitled to rank as "specific" diseases. They may be presumed to be associated with definite changes in the blood, though we have not a right to infer that such changes of state can only be induced in one way. Many well-known chemical changes are capable of being brought about by more than one agency. And just as there is the best reason for believing that cancer or tubercle may be initiated *de novo* by the operation of irritants upon the tissues of certain individuals, and that such growths may subsequently be multiplied within the body by the contact-influence exerted by some of their disseminated particles; so may we suppose, not only that specific substances (contagia) may be capable of initiating specific changes in the blood, but that certain combinations of circumstances may by their action upon the human body entail similar definite changes and states of blood. Having to do with a perverted nutritive activity and mode of growth in a limited area of tissue, cancer or tubercle may make their appearance; whilst, having an altered nutritive activity and set of changes occurring in the blood, this all-pervading tissue may lapse into the successive states peculiar to one or other of the specific diseases, and so give rise to the symptoms by which they are characterised. This is by no means a forced analogy. Can cancer or tubercle arise in the individual without any pre-existing "hereditary taint"? Can the states of blood peculiar to the several specific diseases arise *de novo*, or independently of contagion? These are questions whose import is really similar.†

One of the great and distinguishing peculiarities of these specific diseases is their "contagiousness." Although very differently marked in the several affections, this property is as interesting as it is important. The fact of its existence seems always to have had a large share in determining the nature of the general views which have been held concerning these affections. Even in remote periods, by Hippocrates and others, they were commonly compared to processes of fermentation; whilst since the time of Linnæus, more especially, attention has been often prominently directed to the many apparent similarities existing between the commencement and spread of epidemic diseases, and the "flight, settlement, and propagation of the insect-swarms which inflict blight upon vegetable life."‡ These

* Extracts from Introductory Lecture on Epidemic and Specific Contagious Diseases: Considerations as to their Nature and Mode of Origin. Delivered at University College, October 2, by H. Charlton Bastian, M.A., M.D., F.R.S.

† This double mode of causation is perfectly familiar to the chemist Particular chemical changes may occur under the influence of certain general determining conditions, which at other times (in the absence of these conditions) may be even more easily initiated by a single specific cause. The introduction of a crystalline fragment into a saline solution, and its determination of the crystallisation of all the isomorphous salts contained in the solution, seems to be exactly comparable with the "contagious" origin of diseases. But, under the influence of certain favouring conditions, crystallisation may occur without the contact of a crystalline fragment—the process may be "spontaneous" in the same sense that the occurrence of the blood-change may be "spontaneous."

‡ Sir H. Holland's "Medical Notes and Reflections," 2nd edition, 1840, p. 584. On the following page, the same author writes:—"Connected with these facts is the observation, seemingly well attested, that the cholera sometimes spreads in face of a prevailing wind, and where no obvious human communication is present—a circumstance difficult, if indeed possible, to be explained, without recourse to animal life as the cause of the phenomenon. No mere inorganic matter could be so transferred, nor is vegetable life better provided with means for overcoming this obstacle." Whilst on the preceding page, the "animal species" had been admitted to be "minute, beyond the reach of all sense."

analogies were seemingly strengthened by the increased knowledge which gradually arose concerning the various parasitic maladies to which man and the lower animals were liable. Writing in 1839, Sir Henry Holland says in his essay "On the Hypothesis of Insect-life as a Cause of Disease," "The question is, what weight we may attach to the opinion that certain diseases, and especially some of epidemic and contagious kind, are derived from minute forms of animal life existing in the atmosphere under particular circumstances, and capable, by application to the lining membranes or other parts, of acting as a virus on the human body." Now, the fact of the multiplication of the virus within the body was the peculiarity of these diseases, which, above all others, caused such an hypothesis to be received with favour. Causes which are specific, and which seem capable of self-multiplication—what can such agents be but living things of some kind, plant or animal? This mode of argument was with many all-powerful. And when, after the discovery of the yeast-plant by Schwann, in 1836, new doctrines concerning fermentation began to prevail, the views of those who believed in the living nature of the specific causes of epidemic diseases were in part strengthened. If all fermentations were initiated by the agency of living organisms, and the specific diseases were comparable to processes of fermentation, then how natural was it that many who were moreover influenced by the other analogies, should be led to imagine that the specific causes of these diseases were also living organisms. Only now, attention became directed to the much lower organisms which are so frequently associated with fermentative and putrefactive changes, instead of to insects "minute beyond the reach of all sense."

Here, then, is the origin of what in modern times has been termed "The Germ-Theory of Disease." Like homœopathy and phrenology, this theory carried with it a kind of simplicity and attractiveness, which insured its acceptability to the minds of many. But, however, it seems to rest upon foundations only a little more worthy of consideration than those upon which these other theories are based. Now, owing to its influence, in combination with the more generally received doctrines concerning the origin of life, there has gradually grown up an unwillingness in the minds of many to believe that these contagious diseases can arise *de novo*. And this being one of those theories which tends to curb inquiry, and to check the possible growth of sanitary knowledge in certain highly important directions, it seems to me necessary to look with scrutinising care to its foundations, not only with the view to the advancement of medical science, but with the direct object of removing all checks which may exist to the growth of sanitary precautions against the origin of these most pestilential affections.

Let us see, then, how far the "theory" fulfils the conditions which all good theories do fulfil—how far it explains a great number of the phenomena in question, without being irreconcilable with others.

The advocates of the "germ-theory" have always rested their belief in it, in the main, because they considered that it offered a ready explanation of the increase of the virus of the contagious diseases within the body of the affected person. This increase they suppose is not otherwise to be explained. All other considerations brought forward in support of the theory are just as explicable by another supposition. Fully admitting that the occurrence of a process of organic self-reproduction would be a very adequate way of accounting for the increase of the infecting material, we must see whether this mere hypothesis can be reconciled with other characteristics of these affections. In the first place, it may be asked, whether such a process is actually known to constitute the essence of any general diseases. Because, if so, those in which it does occur, ought, in the event of the hypothesis being true, to present a close similarity to the diseases in which such a process is supposed to occur.

Now, there are certain general diseases which do undoubtedly depend upon the presence and multiplication of organisms in the blood and throughout the tissues generally. There is the epidemic and highly contagious distemper amongst cattle, known in this country by the name of the "blood," and which excites in man that most dangerous morbid condition called "malignant pustule." The researches of M. Davaine * and others have revealed the fact that this disease is essentially dependent upon the presence and multiplication of living organisms, closely allied to *Vibriones*, in the blood of the animals affected, and that similar organisms are also locally most abundant in the contagiously incited "malignant pustule" of man. Unless this latter is destroyed in its early stages, the contained organisms spread throughout the body and the disease speedily proves fatal. Of late, moreover, attention has also been called * to Pasteur's researches on the subject of the very fatal epidemic which raged for fifteen years amongst the silkworms of France. This affection, known by the name of *pébrine*, is dependent upon the presence and multiplication of peculiar corpuscular organisms, called *Psorospermiæ*, in all the tissues of the body. Both these general parasitic diseases are highly contagious; both are contagious by means of organisms; and in both the virus does increase by self-multiplication within the body of the animal affected. What more suggestive evidence could there be as to the truth of the "germ-theory," say its advocates, than is supplied by the phenomena of these two diseases? Undoubtedly the evidence is irrefragable as to its applicability to these particular maladies; but then comes the question whether they are comparable with the other affections to which the "germ-theory" is sought to be applied. And this question must decidedly be answered in the negative. These parasitic diseases are sharply distinguished from the others by the fact of their almost invariable fatality. Creatures or persons once affected in this way are, under ordinary circumstances, thenceforth on the road to more or less immediate death. Happily, however, no fatality of this kind is characteristic of even such highly contagious diseases as scarlet fever and small-pox, or any other of the maladies with which parasitic organisms cannot be shown to be associated. Doubtless there are other general parasitic diseases amongst animals. In almost all the specific diseases to which man is liable, however, I have invariably failed to discover any trace of organisms in the blood. The experience of many other observers has been similar to my own in this respect. But if living things were really present as causes of these maladies, then most assuredly ought they to conform to that fatal type which is almost inseparable from the notion of a general parasitic disease, and which we find exemplified by the course of *pébrine*, the "blood," and "malignant pustule."† The fact then, that the general tendency in the acute specific diseases, is undoubtedly towards recovery rather than towards death, speaks strongly against the resemblance supposed to exist between them and the parasitic affections alluded to, and also against the hypothesis that they are dependent upon the presence of self-multiplying germs within the body. Such germs, when present, would be sure to go on increasing until they brought about the death of their host.

These considerations alone should suffice to inspire grave doubts as to the truth of the "germ-theory." And such doubts may be reinforced by many others. Thus, the several affections being distinct from one another, this theory demands a belief in the existence of about twenty different kinds of organisms never known in their mature condition, but whose presence as invisible, non-developing germs is constantly postulated, solely on the ground of the occurrence of certain effects supposed to be otherwise incapable of occurring. That, if existent, they are no mere ordinary germs of known organisms is obvious, because the presence of these has again and again been shown to be incapable of producing the diseases in question. Mr. Forster says,‡ "There is not perhaps on the face of the earth a human creature who lives on coarser fare, or to a civilised people more disgusting, than a Kalmuck Tartar. Raw putrid fish or the flesh of carrion—horses, oxen, and camels—is the ordinary food of the Kalmucks, and they are more active and less susceptible to the inclemency of the weather than any race of men I have ever seen."§ It has, moreover, been frequently demonstrated, that the organisms of ordinary putrefactions may be introduced even into the blood of man and animals without the production of any of these specific diseases. ‖ Yet is the "Antiseptic System"

* See *Comp Rend* 1864 and 1865

* NATURE, 1870, No. 36, p. 181.

† See paper by Dr. Wm. Budd in *British Medical Journal*, 1863.

‡ See *Med.-Chirurg. Rev.*, 1854, vol. xiii., where the supposed connection of diseases with processes of putrefaction is ably considered by the late Dr. W. Alison.

§ The *Bacteria* which are sure to be abundant in such food cannot, therefore, be the much talked-of "disease germs." Such a diet is, of course, by no means recommended. Epidemic diseases are frequently most fatal when they once break out amongst a people whose diet is of this kind (see Dr. Carpenter, in *Med.-Chirurg. Rev.*, 1853, vol. xi. p. 173), and could probably only be borne in certain climates by persons who lead a very active life.

‖ See, amongst others, Davaine in *Compt, Rend*, August 1864, and E. Semmer in Virchow's *Archives*, 1870. Dr. Lionel Beale is well aware of this fact, and he, accordingly, whilst adhering to the germ theory, promulgates it under a new form. He says (*Monthly Micros. Jour.*, Oct. 1870 p. 205):—"Concerning the conditions under which these germs are produced, and of the manner in which the rapidly multiplying matter acquires its new and marvellous specific powers, we have much to learn, but with

of treatment (good as it may be, irrespective of the germ-theory on which it has been based) pressed upon our attention on the assumption that the germs of putrefaction and the germs of disease are living organisms similar in nature. The strange persistency with which this view is advocated is not a little surprising, when it entails the obvious contradiction that germs which do, under all ordinary circumstances, develop into well-known organic forms, should, when concerned in the production of the diseases in question, induce all the effects supposed to depend upon their prodigious growth and multiplication, and yet never develop, never become visible. And, whilst *Bacteria* and other organisms with which the unknown disease-germs are compared, flourish and reproduce in the much-vaunted, germ-killing, carbolised lotions;* still carbolic acid continues to be recommended solely on account of its germ-killing powers, and the theory on which the practice is based is thought to derive support from the results obtained by the use of this agent. Surely no theory could be weaker on which to base a successful method of treatment; and if, as its distinguished originator says,† its general acceptance is principally hindered by the "doubt of its fundamental principle," then I would deliberately say that the blame, if any, cannot fairly be said to lie with those "who have opposed the germ-theory of putrefaction." The "Antiseptic System" of treatment needs no support from a germ-theory; it can be surely and unassailably based upon the broader physico-chemical doctrines of Liebig.‡

The last blow, however, seems given to the "germ-theory" of disease, when we are told that the blood and the secretions in sheep-pox are not infective, though this disease is most closely allied to, and even more virulently contagious than, human small-pox. If germs had existed in this general disease, and their multiplication was the cause of it, then most assuredly would they have existed in the blood and in other fluids of the body; and yet, as Prof. Burdon Sanderson tells us,§ "In sheep-pox all the diseased parts are infecting, while no result follows from the inoculation either of the blood or of any of the secretions; the liquid expressed from the pulmonary nodules has been found by M. Chauveau to be extremely virulent—certainly not less so than the juice obtained from the pustules." Now, although in other of these diseases the blood does undoubtedly exhibit infective properties, still the ascertained existence of even one exceptional case amongst maladies so contagious as sheep-pox, seems to be absolutely irreconcilable with the theory of the "germ-theory," more especially when this theory was started principally to explain the phenomena of such highly contagious diseases.‖

vegetable organisms the germs have nothing to do. They have originated in man's organism. Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy, and self-sacrifice may succeed in extirpating them, and may discover the means of preventing the origin of new forms not now in existence." This is undoubtedly a very much less objectionable form of the germ theory, though much additional evidence would be needed before we could accept the view that contagious diseases are due to the rapid multiplication of the contagious particles within the body of the creature affected. The non-contagiousness of the blood is as irreconcilable with this as with the other form of the germ theory.

* See "Modes of Origin of Lowest Organisms," 1871, p. 85. And in a recently published paper "On the Relative Powers of Various Substances in Preventing the Generation of Animalcules on the Development of the Germs," Dr. Dougall says: "If, as is alleged, germs are the source of putrefaction, then the strongest preventives *must* be the best antiseptics, and *vice versâ*. . Now, as seen in the table, carbolic acid occupies a very mediocre place as a preventive, therefore it is legitimate to conclude that it stands no higher as an antiseptic," p. 13.

† *British Medical Journal*, August 26, 1871, p. 225.

‡ These doctrines do not seem to have been adequately grasped by Prof. Lister. Fragments of organic matter are believed by Liebig to be capable of acting as ferments; he, however, holds that their potency is deteriorated by heat almost as much as are the qualities of living ferments. The experiments with boiled fluids in bent-neck flasks, therefore, upon which Prof. Lister so strongly relies in proof of the germ-theory, prove absolutely nothing as between the two theories of fermentation of Liebig and of Pasteur. Amongst the atmospheric particles there are sure to be dead ferments in the form of mere organic fragments. Now the doubt that previously existed was, as to whether they could initiate fermentation and putrefaction, or whether the presence of living germs was absolutely essential. In the experiments with bent-neck flasks, both fragments and germs must be simultaneously excluded or admitted to the fluids. Prof. Lister's readers might suppose that Liebig had no objection to his ferments being boiled, and that the issue lay between the relative efficiency of oxygen and living germs. (See Gerhardt's *Chimie Organique*, t. iv. p. 545.)

§ Report "On the Intimate Pathology of Contagion," in Twelfth Report of Medical Officer of Privy Council.

‖ Inoculation with the blood of a person suffering from measles has also in several cases failed to reproduce the disease. The different severity of small-pox taken in the ordinary way, and that induced by "inoculation" of the matter of a small-pox pustule, is also quite inexplicable in accordance with the "germ theory."

Dr. Bastian tabulates the whole of the communicable diseases in the following manner:—

COMMUNICABLE DISEASES.

	PARASITIC DISEASES AFFECTING:		
Many of them capable of arising "de novo."	External (cutaneous) surface. Internal (mucous) surfaces. Closed (serous) cavities. Tissues of organs or parts. (*Psorospermiæ, Cysticerci Nematoids, etc.*) Blood. (*Bacteridia* in 'Malignant Pustule,' *Psorospermiæ* in '*pébrine*,' etc.		Caused and propagated by the presence and self-multiplication of living units.
	TISSUE DISEASES. A. *Diseases of Internal Formed Tissues and of Mucous Membranes.*		Caused and propagated by chemico-physical agencies, and not by the multiplication of living units.
All inoculable and capable of arising "de novo."	Fibro-plastic growths. Cancerous growths. Tubercular growths Glanders. Syphilis. Gonorrhœa. Purulent ophthalmia. Diphtheria and Croup	*Principally Sporadic.*	
	B. *Diseases of the Blood* (*principally*).		
All contagious and capable of arising "de novo."	Erysipelas. Puerperal fever. Surgical fever. Pyæmia. Hospital gangrene. Rabies.		
	Rheumatic fever. *a.* Dengue. *b.* Sweating sickness Intermittent fever. *a.* Remittent fever. *b.* Yellow fever.	*Principally Endemic.*	
Contagiousness either absent, little marked, or more or less virulent; all probably capable of arising "de novo."	Summer diarrhœa. *a.* Choleraic diarrhœa. *b.* Cholera. Dysentery. Influenza. Mumps. Relapsing fever. Typhoid fever. Typhus fever. *a.* Cerebro-spinal menningitis? *b.* Plague. Varicella. Hooping cough. Measles. Scarlet fever. Small-pox.	*Often Epidemic.*	

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ENGLISH.—The Subterranean World: Dr. G. Hartwig (Longmans and Co.).—On the Use of the Ophthalmoscope: T. C. Allbut (Macmillan and Co.).—Rudimentary Treatise on Geology; Part I., Physical: Ralph Tait (Lockwood and Co.).

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